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SUBJECTIVE ASSESSMENTS OF FIRMNESS: THE USE OF A RATING SCALE

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An experiment was carried out to investigate the extent to which an absolute standard for firmness could be held independently of variations occurring within the samples being judged.

Although the method gave good results as a whole, it was found that, when correlating the ratings to the objective measurements, the absolute standards were not held independently of the average firmness of the groups being judged. In practice, if a certain sample was judged, within a "soft" series of samples, it would be rated as being firmer than when it was judged among "firm" samples. Certain differences in the effects of these changes were related to the nature of the subjects' previous experience.

When the results were considered on a relative comparison basis, it was found that a much lower threshold was achieved than had previously been obtained with judgments for springs using paired comparison methods. There were certain trends in the levels of discrimination with time. These trends are discussed.

The strength of grip of the subjects bore some relation to their initial rating values, but apparently was not related to their level of discrimination. Subjects who were able to exert a more constant pressure throughout the test did not seem to be any better at discriminating than were others.

Ratings made for a single sample two months after the main experiment were very similar to those previously given for the same sample.

Introduction

In an earlier paper, Sheppard and Scott-Blair (1952) reported some studies in which attempts were made to assess the firmness of spring samples by the use of "absolute methods." It is intended in this paper to report further experiments, making use of these methods.

Absolute methods are of particular interest, since the ultimate purpose of these researches is to study quality assessments of cheese and other dairy materials, and rating methods are commonly used for this purpose.

A study of ratings made for the firmness of Cheshire cheese (Sheppard, 1952) had suggested that the judges were unable to maintain their absolute standards independently of the average (objective) firmness of the groups being judged, and it was considered advisable to try to confirm this finding using materials for which more adequate objective measurements were possible.

It was reported in the latter paper that three judges, of whom two were experienced in assessing cheese qualities, were unable to give consistent ratings for firmness when there were variations in the general level of firmness of the groups being judged on any one day. The effect of the general level of firmness of the group being judged was such that, if two cheeses, objectively of the same firmness, were presented independently in two separate groups of cheese which differed in firmness, then the

one presented with the softer group of cheese would be rated as being firmer than would that in the firmer group.

Tresselt (1948) reported similar findings in that groups of subjects, despite the nature of their previous experience, soon started giving absolute judgments adapted to the range of stimuli being presented. The main difference between her experiments and the type of situation occurring in grading, such as that described by Sheppard, is that her subjects were not instructed to maintain their absolute values constant in terms of the stimuli presented. Whereas, of course, it is essential for a grader to be able to keep his mental standards constant when he can have no objective standard as a basis for comparison.

Tresselt (1947) has reported an experiment in which some of her subjects were more experienced than others, and it was decided to include this feature in the present experiment. Besides this, the subjects were chosen so as to differ in the recency, as well as in the amount, of their previous experience. Tresselt had tried introducing one large change in the stimulus intensities, but in the present experiment there were several changes in the average firmness of the samples as the experiment proceeded, and the changes were not all in one direction. This led to a more real-life situation in terms of grading procedures, while in addition the subjects were asked to rate the samples on a 1–7 scale for firmness, this being the procedure used in the experiments on assessing the firmness of Cheshire cheese.

Since earlier work, not yet reported, had led the experimenter to think that certain errors in discrimination might result from changes in the pressures exerted when compressing the samples between the finger and thumb, the subjects recorded their pressures on a finger dynamometer at the end of each experimental session in Part I.

Part I: The Experimental method

Eighteen subjects were used, of whom one was unable to complete the whole test. Five of these had had considerable experience (some of it recent) in judging firmness of springs and rubbers; another five, including the subject who did not complete the series, had had less experience with these materials, and no very recent experience; the remaining eight subjects had not previously attempted such a task.

The subjects did the test at the same time on each day as far as this was possible; the task being to rate four samples on a 1–7 point scale for firmness. One point was to be given for "very firm," 4 points for "average," and 7 points for "very soft." The samples were the steel springs faced with rigid plastic discs, described in the earlier paper. The samples were bound with different coloured ribbons, the same ribbons being used on each day; the subjects were warned, however, that the ribbons would not give any clues as to the firmness of the samples.

The subjects were told that they could estimate the firmness of the samples in any way they chose, although it was preferred that they should not look at the samples while they were squeezing them. They were to handle the samples as many times as they liked, and to compare one with another if they wished to do so.

They were instructed that the samples which would be given to them would vary over quite a wide range throughout the whole experiment, and that they were to try to ensure that any one rating value which they might give would be allotted to samples which were of about the same firmness throughout the experiment.

Fifteen different samples were used altogether, four being judged on each of 10 successive days, with an interval of two days between the fifth and sixth day. The same four samples were judged by all subjects on each day; the four to be judged being laid out on a table so that they could be compared in any way the subject chose.

When each subject had completed the task to his satisfaction, he recorded seven "normal" pressures on a finger dynamometer.

At intervals throughout the experiment the subjects were asked, as a check, whether they believed that they were maintaining their rating values constant.

The firmness of the samples was varied as shown in the Table, this plan being based

on the changes in firmness occurring in the experiment on Cheshire cheese reported by Sheppard (1952). Briefly, the samples started by being soft, became progressively firmer for eight days, and then tended to become softer on the last two. The change in the levels was not regular, however.

Finally, two months after the main experiment, the 17 subjects who had completed the test were asked to rate one sample, in fact one of the firmest previously used, on the same 1–7 scale. They had not been informed that they would be asked to do this.

PART I: RESULTS

The results were examined in three different ways. First as an experiment on absolute judgments, secondly, as a method of relative comparisons, while finally, the relationship between the pressures recorded and the subjects' ability to discriminate was considered.

A. *The experiment as an absolute method.*

The ratings on each sample were averaged for the three groups of subjects, and these ratings considered separately for each group. The ratings for all 18 subjects were grouped and are given in the Table.

In this Table the serial order of presentation is set out against the objective firmness, as measured by the amount of compression of each sample under a constant load. The spring samples presented (C-T) are shown at the side in the appropriate positions according to their firmness.

At the head of each column the average firmness of the four samples judged is indicated and this mean value is also represented by two arrows crossing the column. Then in each column the averaged ratings for all the subjects are inserted opposite the code letter of the samples which were presented on that day. A high objective, or subjective, value indicates that a sample is a soft one.

It can be seen from the Table that the ratings are not independent of the average firmness of the series, and that, in general, the same sample will, if judged in two successive groups of differing average firmness, be rated as being firmer when it is judged within the softer group of the two. Thus samples T, Q and R were rated as being softer in group II, than they were in group I, where the group was, on the average, somewhat firmer, likewise sample D is rated softer in group VIII than in group IX, where the latter group is, on the average, softer.

The table shows clearly how the average rating can vary for any one sample; thus sample P was given values from 2·4 to 5·1, and Q from 3·2 to 5·4. However, when samples were rated within groups which were similar in average firmness, the ratings allotted to them remained very constant, thus sample L was given ratings which varied from 3·1 to 3·5 only.

The effect of the change in firmness of the group on the rating for any one sample, is so precise in certain cases, that one could almost predict the change in value from the change in the group firmness.

This is particularly true for the non-experienced group, which appears to be more precisely affected than the others. On the other hand, although experience does seem to have been important, in that the non-experienced, the semi-experienced and the expert group are affected most precisely, in that order, by these changes; it is, in fact, the semi-expert group which shows the greatest changes in rating value, when there is a change in the average firmness of the samples, the non-expert group being slightly less affected, and the expert group least affected of all. It does seem, therefore, as if the experience of the more expert groups may have enabled them to resist to some extent, the influence of the "background" against which their judgments were made, although there is not sufficient data to guarantee this.

TABLE TO SHOW HOW THE SUBJECTIVE ASSESSMENTS FOR FIRMNESS CHANGE IN RELATION TO THE FIRMNESS OF THE SERIES BEING JUDGED

(Intervals of a day between the judgments on each group)

	GROUP		I	II	III	IV	V	VI	VII	VIII	IX	X	
			Average firmness of group										
Sample Name	Firmness in objective units		2·61	2·53	1·85	2·15	1·82	1·85	1·99	1·37	2·04	2·03	Two months later
↑ SOFT T	2·85 — 2·89		5·2	5·6	—	—	—	—	—	—	—	—	—
	2·80 — 2·84		—	—	—	—	—	—	—	—	—	—	—
S	2·75 — 2·79		4·7	—	—	—	—	—	—	—	—	—	—
	2·70 — 2·74		—	—	—	—	—	—	—	—	—	—	—
	2·65 — 2·69		—	—	—	—	—	—	—	—	—	—	—
	2·60 — 2·64		→ ←	—	—	—	—	—	—	—	—	—	—
Q	2·55 — 2.59		3.2	4·1	—	—	—	—	5·4	—	5·0	—	—
	2·50 — 2·54		—	→ ←	—	—	—	—	—	—	—	—	—
R	2·45 — 2·49		3·2	4·6	—	5·1	—	—	—	—	5·6	5·6	—
	2·40 — 2·44		—	—	—	—	—	—	—	—	—	—	—
	2·35 — 2·39		—	—	—	—	—	—	—	—	—	—	—
	2·30 — 2·34		—	—	—	—	—	—	—	—	—	—	—
	2·25 — 2·29		—	—	—	—	—	—	—	—	—	—	—
P	2·20 — 2·24		—	2·4	5·1	3·6	—	5·1	4·9	—	—	4·6	—
N	2·15 — 2·19		—	—	—	→4.3←	5·0	—	—	—	—	—	—
	2·10 — 2·14		—	—	—	—	—	—	—	—	—	—	—
	2·05 — 2·09		—	—	—	—	—	—	—	—	—	—	—
	2·00 — 2·04		—	—	—	—	—	—	—	—	→ ←	→ ←	—
	1·95 — 1·99		—	—	—	—	—	—	→ ←	—	—	—	—
	1·90 — 1·94		—	—	—	—	—	—	—	—	—	—	—
	1·85 — 1·89		—	—	→ ←	—	—	→ ←	—	—	—	—	—
FIRM L	1·80 — 1·84		—	—	3·5	—	→3·5←	3·2	3·4	—	—	3·1	—
M	1·75 — 1·79		—	—	3·4	3·5	3·5	3·8	—	—	3·4	—	—
K	1·70 — 1·74		—	—	—	2·1	—	—	—	—	—	—	—
	1·65 — 1·69		—	—	—	—	—	—	—	—	—	—	—
J	1·60 — 1·64		—	—	2·1	—	—	2·4	—	—	—	2·7	—
H	1·55 — 1·59		—	—	—	—	1·8	—	—	3·4	—	—	—
G	1·50 — 1·54		—	—	—	—	—	—	—	—	—	—	—
	1·45 — 1·49		—	—	—	—	—	—	—	—	—	—	—
	1·40 — 1·44		—	—	—	—	—	—	—	—	—	—	—
D	1·35 — 1·39		—	—	—	—	—	—	2·1	→2·6←	1·8	—	2·85
F	1·30 — 1·34		—	—	—	—	—	—	—	2·6	—	—	—
C	1·25 — 1·29		—	—	—	—	—	—	—	2·1	—	—	—

Despite these effects, however, relating the 40 ratings for the samples to the equivalent objective measurements, for each group of subjects, produced correlation coefficients of +0·82 for the expert group, +0·77 for the semi-expert group, and +0·81 for the non-expert group. (All significant at a 0·001 level.) The correlations for these groups are not significantly different from each other, but it is worth noting that these differences in "accuracy," as shown by these correlations, are apparently less related to differences in discriminatory skill, between the groups, than to the extent to which the different groups were affected by the average firmness of the samples on each day. Differences in ability to discriminate, for the different groups, will be discussed further in section B.

It was of interest to note that, averaging the rating made by all the subjects on the first day of the experiment, gave a value of 4·1, almost exactly the scale value for "average firmness." There were considerable variations from this by different subjects, but the only group who differed at all markedly from the "average firmness" value was the expert group, which gave an average rating value of 4·4. This was not significantly different from the values given by the other two groups, but it indicated that the first group of samples were softer than usual, which was, in fact, the case. It was noted that the members of the expert group agreed less between themselves about the average firmness of these samples, than did the members of the other groups, and this might indicate that their experience gave them sufficient confidence to give judgments which were further removed from an "average firmness" value.

Some subjects in the experienced groups had had previous experience in making judgments for the complete range of springs used in this experiment, while some had only had experience with the firmer springs in the series. It was thought that this might possibly influence the initial ratings of the subjects. In fact, the difference between the initial ratings, made by these two sets of subjects, was contrary to that which had been expected; the group which had only dealt with firm springs giving an average rating of 3·8, while the group which had had experience with the whole range gave an average rating of 4·5. The differences were not significant, however. It was a period of months since the softer springs had last been used, and it is possible that the strength of the subjects may have been more important in deciding the initial values allotted to the first group of samples. The latter point will be discussed in section C.

The standard deviation for the average ratings by the 18 subjects, on each day, was calculated. In general, the S.D. was about 0·8, but on the second day it increased to 1.3. It is tempting to suggest that this unusually high deviation between the subjects' average ratings was a result of some sort of confusion in their mental standards before becoming adapted to the general level of firmness, and when trying to break away from giving judgments tied closely to "average firmness."

The average rating for sample D, judged two months after the main experiment, is shown in the last column of the Table, and it can be seen that it was very similar to those given previously. The difference between the average of the three assessments in the main experiment, and this later assessment is significant at a 0·10 level only.

Sample D was rated as being slightly softer than it had been in the main experiment, and it is likely that this was a result of the "central tendency" previously noted, in that subjects tended to give judgments approximating to the average of the 1–7 scale, when they were uncertain of the true value.

The majority of the subjects claimed that they were fairly confident of their judgments, even after the two months' interval, and the deviation of the ratings was in fact, somewhat less than it had been on the previous three occasions.

The subjects were asked, as a matter of interest, whether they had any sort of "image" of the kind of firmness that they expected for a given rating. In fact, only one reported affirmatively, the rest stating that when they handled a sample, they could then tell how firm or soft it was, but they did not have any sort of image when they were not handling a sample.

B. Relative comparisons within the groups.

In an attempt to get a threshold for relative comparisons, when allowing the subjects to handle the samples as they wished, the accuracy was estimated for each of the six relative comparisons made on each day, by the rating of the four samples.

"Equal judgments," occurring when the same ratings were given for the two different samples, were left out of these calculations, since it was not possible to say definitely that such judgments were wrong, as one position on the rating scale could be extended to cover a considerable range of firmness, nor could these judgments be classed with certainty as being correct.

Plotting the per cent. correct judgments against the objective firmness ($\triangle$n) as in the earlier paper, gave a threshold at a 75 per cent. correct level of 9 per cent. $\triangle$n. This was a lower threshold than any previously achieved for judgments on spring samples, and the same level as that achieved when comparing rubber cylinders by the method of paired comparisons.

It did seem as if the non-expert groups were rather better at discriminating than were the experienced groups, and the apparent lowering of the thresholds may be partly due to this. However, the difference between the groups' acuity was probably not significant, and plotting the data from the more experienced groups separately, increased the threshold only to a $\triangle$n of 10 per cent.

It seems, therefore, that allowing the subjects to estimate the firmness of the samples in any way they chose enabled them to discriminate more accurately than by even the best "controlled" methods described in the earlier paper. It is also possible that the difference in acuity might result from the subjects having to compare some four cylinders instead of making comparisons between 28 pairs, as in the experiments previously reported. In fact, however, there did not seem to be any evidence that the subjects became less efficient throughout the course of a single paired-comparison test, so the former hypothesis is more likely to be correct.

There was a further finding of considerable interest when examining these relative comparisons. On several occasions throughout the ten days of the whole experiment the same two cylinders had to be rated by the subject. It was thus possible by comparing the percentage of correct comparisons on these different occasions to see whether the subjects improved, or became worse, at discriminating as the experiment proceeded.

It had been expected that a gradual improvement in discrimination might take place as the test proceeded. In fact, however, the effect of repeated tests was more complex. It was found that, when there were large objective differences between the two samples, all the subjects made the relative comparisons correctly throughout, and that when the differences were slightly less large, the relative differences were judged somewhat more accurately later in the 10-day period than they were at the beginning.

For smaller objective differences, however, at less than the 80 per cent. threshold level, discrimination seemed to deteriorate with time. This was in itself surprising, in view of the improvements in discrimination for larger objective differences, but it was even more surprising that when the objective differences were very small ($\triangle n = 0{\cdot}7$, and $3{\cdot}4$) the number of correct judgments approximated very closely to

to the 50 per cent. (chance) level, at the outset of the experiment, and later fell markedly below this level, so that it seemed as if the subjects improved in giving the wrong response.

Since it was thought necessary to exclude "equal" responses, the percentage of correct totals was based on rather small numbers of subjects, so the differences between the numbers of correct responses at different periods, for any one objective difference, in the experiment, were not significant. Nevertheless, although there were not significant data to allow a definite conclusion, it is tempting to enquire why these changes took place.

A theory of boredom, or fatigue, to explain the occurrence of decreases in correct discrimination seems unsatisfactory in view of the increased percentage of correct judgments for the pairs with larger objective differences. This leaves two possible explanations; the first is that, in fact, there was an increase in discriminatory skill, but that for the differences below 50 per cent. the subjects for some reason improved at giving wrong judgments; while possibly the apparent decreases in acuity for $\triangle$n's lying between the 80 per cent. threshold and the 50 per cent. (chance) level resulted from a similar change in ideas. This does not seem to be a very probable theory, but the other possibility, that the subjects were in fact basing their judgments on some other difference between the samples, when there were only small objective differences in compression modulus, (Harper, 1950, 1952) seems unlikely, on physical grounds, to be the explanation with the spring samples used in this experiment. It is worth recording here, however, that earlier work with paired comparison methods had shown that for certain pairs of spring samples with small objective differences, significant negative discrimination, in terms of compression modulus, did occur.

C. Relation between finger strength and discrimination.

Harper (1950, 1952) has suggested that there was likely to be a connection between ability to maintain a constant finger pressure throughout any given test, and ability to discriminate for firmness.

In fact there was no relation between the ability of the subjects to maintain constant pressures, either on any one day, or over the period of 10 days, and their ability to discriminate effectively between pairs of samples. This finding is in agreement with the results of similar research by McKennell (1951). Neither was there any relationship between the relative skill of the 18 subjects and their strength of grip ($r = +0{\cdot}01$) as recorded on the finger dynamometer.

There was, however, a positive correlation ($r = +0{\cdot}37$) between strength of grip and the average rating given on the first of the 10 days. This is only significant at a 0·10 level for this number of subjects, but it does suggest that the subjects who exerted a stronger pressure between finger and thumb, tended to rate the samples on the first day as being "softer," than did the other subjects who were weaker. It was thought that this effect might appear more marked for the non-experienced group than with the others, but this was not the case.

There was also a very small correlation ($+0{\cdot}11$) between the average strength of each subject, throughout the whole experiment, and the average rating value throughout the whole test. This was not significant even to the 0·10 level, but it is likely that the correlation would have been reduced by the observed changes in the rating values used by subjects during the course of the experiment.

An analysis of variance was made for the average strength for each subject on each day. There was a significant difference in pressures exerted between subjects (0·001 level) and the difference between days, for the subjects, was also significant (0·01 level).

An analysis of variance was also made for differences within testing periods of seven pressures, and this showed significant differences between these. It seemed as if the pressures showed some tendency to become progressivly weaker throughout each test period, although a regression coefficient did not show any significant trend with time. Such a trend would have been expected to occur from previous work carried out by the author, and from some research published by MacPherson, Dees and Grindley (1948) although the latter found that pressures tended to increase progressively with time.

There was no apparent relationship between strength of grip and the ratings for sample D made after the main experiment.

Part II

The experiments described in Part I had shown that the absolute standards were not held independently of the groups being judged, when there were long intervals between the judgments on each group of stimuli. It seemed likely, however, that if the time interval between each group of judgments was shortened, the subjects might be less influenced by the average firmness of the groups. Two experiments were therefore carried out to test this.

The experimental method.

The method was the same as in Part I, the only difference being that in one experiment there was an interval of 55 minutes between the judgments made on each group, and in the other there was an interval of 15 minutes. Eight subjects were used in each experiment, all of whom were new to this type of task.

The finger dynamometer was not used in these experiments, since this extra task might have caused some fatigue in the subjects' fingers, with these short intervals. Otherwise the plan was the same, four samples being presented in each group, with their firmness varying as shown in the Table.

A. The experiments as absolute methods.

Despite the shorter time-intervals, the judgments seemed to be just as much affected by the average firmness of the groups, within which the samples were presented, as in the first experiments.

Comparing these results with those for the non-expert group in Part I showed that it would have been possible to predict the changes in rating values, from the changes in the group average firmness, most precisely when there were intervals of 15 minutes, somewhat less precisely with intervals of one day, and least precisely for 55-minute intervals.

The amount by which the rating values altered was similar for each of the three time-intervals.

The correlations between the objective measurements, and the subjective ratings, were $+0{\cdot}75$ for the experiment with 15-minute itervals, and $+0{\cdot}83$ when there were 55-minute intervals. These are similar to the values given in Part I for the experiment with longer time intervals. The fact that, in the 15-minute interval experiment, two subjects thought the samples were the same for each group, probably accounts for this correlation being lower than for the 55-minute interval experiment.

B. Relative comparisons within the groups.

Threshold values were calculated, as in Part I, and were found to be about 9·5 per cent. $\triangle$n. This was a slightly higher threshold than that for the non-expert group in Part I, but was still lower than that for the expert group.

Trends in discrimination, similar to those described in Part I, also occurred in these two experiments.

Conclusion

These experiments have shown how absolute standards can be affected by the "frame of reference" within which judgments have to be made, and that the effect of the latter may be so precise as to make it possible to predict accurately the changes in rating values that will be given for identical samples. Furthermore, "the frame of reference" will affect the rating value just as much when the interval between judgments on each group is as short as 15 minutes, or as long as one day.

Nevertheless, it may be claimed, as Wever and Zener (1928) suggested, that judgments made in terms of absolute standards are useful, and can give quite good results.

The judgments made on a single sample after the main test in Part I, without any frame of reference, were interesting, and it is suggested that such a method should be used to test the efficiency of absolute judgments unaffected by relative comparisons. It is intended to carry out further experiments on this.

The change in levels of discrimination was an interesting feature, and this, together with other problems, such as the relations between finger strength and judgments for firmness will be investigated further.

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THE INTERACTION OF HUNGER AND THIRST IN THE RAT*

BY

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Two groups of rats were given a series of trials in an enclosed runway with a food reward at the end, one group being run hungry, the other hungry plus thirsty. Then each group was split into three sub-groups: one run hungry, the second thirsty and the other hungry plus thirsty, in each case without food reward.

It was found that, whereas on the rewarded runs the extra, "irrelevant," thirst increased running speed, on unrewarded runs it had the opposite effect and slowed up performance. Thus on unrewarded runs the two sub-groups running thirsty, and hungry plus thirsty, ran as slowly as those running hungry. Differences were found not to depend on whether the animals had been hungry or hungry plus thirsty on previous rewarded runs.

The interaction of primary needs therefore depends on the external situation. This can be accounted for in terms of the Pavlovian theories of mutual induction and conditioning, but not in terms of Hull's theory of "irrelevant drives."

I

Introduction

As used by animal psychologists the term drive generally implies a splitting of underlying processes into a driving and a driven part; into a piece of inert machinery on the one hand and the energy setting this machinery in motion on the other. The basic model is very well elucidated by Woodworth's statement about how he came to introduce the term into psychology: "I got it from mechanics. A machine has a mechanism, such that if it is put in motion it operates in a certain way; but it must be driven in order to move. The 'drive' of a machine is the supply of energy that puts it in motion." (Quoted by P. T. Young, 1936, p. 71.)

The recent expression of this view of the processes underlying behaviour is contained in Hull's theory of primary motivation (Hull, 1943), according to which the inert machinery consists of receptor-effector connections which have to be brought into action potentiality by certain chemicals in the blood supplying these connections. These humoral factors provide the drive which sets the machinery in motion. Basically similar, though less explicit, notions will readily be found among other psychological theorists.

For this type of theory the question of the mode of operation of drives resolves itself into firstly, their relation to habits or "associations" and secondly their relation to other drives.

These theories conceive the habit-drive relation as a one-way process in which drive activates habit, but not vice versa. On the question of drive interaction Hull's view approximates to that of an arithmetical summation of all the drives active at any moment, whether relevant to the reward or not.

There has more recently developed a theory of needs which differs from Hull's in several important respects. The tendency to treat the nervous system as a complex of inert machinery needing an external mover to set it in motion has given way to an appreciation of the importance of central factors in the physiology of needs. These conceptions have been outlined by C. T. Morgan (1943, Chapter 22), who describes a central motive state, or c.m.s. with the following properties: a maintained level of excitation, connection with motor centres leading to a specific reaction

* The experimental work reported in this paper was performed at the Institute of Experimental Psychology, Oxford.

and lowering of specific stimulus thresholds. This is to be distinguished from the humoral motive-force which probably acts to increase the level of excitation in the c.m.s. The logical development of these views is to be found in Hebb's theory which can dispense with the concept of drive as a moving agent altogether and which uses such terms as motivation only to refer to certain *properties* of central processes and not to entities (Hebb, 1949, Chapter 8).

These theories have not so far been concerned with the problem of need interaction. To discover how this problem may be approached from a centralist point of view we have to turn to the now classical work of I. P. Pavlov. As long ago as 1910 Pavlov (1928) postulated a food centre with properties not dissimilar to Morgan's central motive state. In addition he shows that the activity of this centre is constantly modified by stimuli both from without and from within the organism. Numerous examples of the former are given and it is shown that the food centre obeys the same laws as the centres for conditioned reflexes. Its action may be inhibited by the action of other centres or it may in turn inhibit other centres, depending on the level of excitation in each. An illustration of this reciprocal action of centres is given by means of Yerofeyeva's experiment, using electric shock as a conditioned stimulus for food. The activity of the food centre completely inhibits any defence reaction, except when the shock is very severe (Pavlov, 1928, p. 186).

When a "temporary connection" is established between two centres, excitation may flow from one to the other so that the action of conditioned stimuli can augment the excitation of unconditioned centres and vice versa. A complex example of the former case is mentioned by Pavlov (Ch. 25, Lectures, Vol. I) where the food response is balanced against the so-called "guarding" reaction. As more and more conditioned food stimuli are presented to the animal, the excitation in the food centre grows demonstrably, until finally the "guarding" reaction is completely inhibited and the animal eats. The outcome of the struggle between unconditioned centres, therefore, depends on the nature of the external stimuli reaching the animal.

The present experiment was designed to test the action of an "irrelevant" need: (*a*) under conditions where the external stimuli favoured the "relevant" need, and (*b*) under conditions where this was not the case. Hunger was used as the "relevant" need, and thirst was "irrelevant" to the food reward.

The experimental conditions thus fall into two parts. One in which the food reinforcement is provided, and in which, therefore, the food centre receives additional activation from conditioned stimuli, and a second part in which the reinforcement is withdrawn and the stimuli therefore acquire an inhibitory action.

A certain complication of the experimental design will allow the testing of a further hypothesis about drive action as conceived by a peripheralist theory like that of Hull. This concerns the conception of drive as a local stimulus, produced by stomach contractions, for example, which becomes part of the stimulus complex to which the overt response has been conditioned. This is therefore a second way in which drives influence habits.

The importance of this feature of drive action could be assessed in this experiment by comparing on their non-rewarded runs those animals in which thirst had been present during training and those in which it had not been present, i.e. which had been used as controls in the first part of the experiment.

II

Apparatus

The Apparatus consisted of a simple runway 5 ft. long and 5 in. wide with sides of plywood 1 ft. high and a perforated zinc floor. At either end this runway was attached to an enclosure 9 in. × 10 in. × 12 in. (For further details *see Danziger*, 1951.)

Subjects

Thirty-six male hooded rats of a strain bred by the National Institute for Research in Dairying at Reading and taken over by the Department of Human Nutrition, Oxford. They were all three to four months old at the start of the experiment. One animal became ill during the experiment, so that the analysed data are based on only 35 animals.

Experimental Design.

The experiment divides into two periods, the training runs, when a food reward is provided at the end of the runway, and the test runs when no such reward is given. During the training runs one group of 18 animals is hungry only, while a second group of 18 is hungry plus thirsty. On the test runs which follow each of these groups is subdivided into three smaller groups of six animals each. One of these sub-groups is hungry only, a second is thirsty only and a third is hungry plus thirsty. There are therefore 12 animals under each of the conditions of motivation on the non-rewarded, test runs, but they can be subdivided into groups of six on the basis of their previous motivation during the rewarded, training runs. In other words an analysis of variance can be performed on the scores of the test runs to compare the contribution of the two components: motivation during training runs and motivation during test runs.

Procedure.

For a fortnight before the first preliminary trial the rats were all handled daily by the experimenter in order to tame them. At the same time their food in the form of dry rat-cake nuts was supplied only once a day for 90 minutes, at the end of which period it was removed from their cages. In this way a 24-hour feeding schedule was established and this was maintained throughout the experiment. "Hungry" in this experiment means 22 hours after the end of the feeding period of the previous day.

Preliminary trials consisted of four five-minute periods of free exploration in the apparatus on successive days. These took place 1–2 hours before the daily feeding period. In this way the rats were familiarised with the experimental situation. On the fifth day each animal was fed one of the reward pellets by hand in its home cage and was then placed directly in the end box of the apparatus where more reward pellets and rat-cake nuts were placed on a dish. The door to the runway was closed and the rats were kept in the end box for three minutes. If they had not begun eating by that time, which almost all of them had, they were allowed another two minutes in which to begin eating. By this time all ate.

On the training trials which followed, each of the animals was given two runs a day to a food reward. The first run took place about 45 min. before the daily feeding period, and the second run followed after an interval of 30 min. On the first day of reward, however, only one run was given. This procedure was continued for 10 days, so that at the end of that time all the animals had received 19 rewarded runs.

At the beginning of the experiment the animals had been randomly assigned to their groups and sub-groups. Group A was run hungry and thirsty (17 hours water deprivation) on rewarded runs while group B was hungry only. After the 10 days of rewarded runs each of the groups was divided into its three sub-groups, each of which was treated differently. In each case one sub-group had its test trials under 22 hours food deprivation only, a second sub-group was under 17 hours water deprivation only and a third sub-group was under 22 hours food plus 17 hours water deprivation at the start of the test trials. Those groups which were run under water deprivation only received a ration of slightly moistened rat cake nuts in the form of a mash one hour before their experimental runs were due to begin. This should have considerably inhibited the activity of the food centre, without unduly reducing thirst.

On the day following the 19 rewarded runs, each group received six non-rewarded trials which were held at four-minute intervals.

As has been stated, a water deprivation period of 17 hours was used to produce thirst. This was accomplished simply by removing the water bottles from the cages at the appropriate time. In the case of those groups which were running hungry but not thirsty, water bottles were removed three hours after the daily feeding period and reintroduced one hour before the daily trials were due. This ensured a considerable amount of drinking just before these trials.

The method of measuring performance at each trial was as follows. At the beginning of each trial the rat was dropped into the starting box facing the wall opposite the runway entrance. As the rat touched the floor of the starting box the experimenter started a

stopwatch equipped with two hands and stepped back behind the starting box to a position in which he was invisible to the rat, but from which he could just observe the entrance to the alley. When both the rat's forelegs were actually in the alley and out of the starting box the first hand of the watch was stopped. The measure obtained was therefore one of the speed with which the rat reacted to the starting box by setting off on its run down the alley. When the rat had three feet inside the end box, the second hand of the watch was stopped so that the difference between the first and the second reading gave a measure of the time taken by the rat to traverse the 5-ft. runway. Time readings were taken to an accuracy of 0·1 sec.

On rewarded trials the rats were allowed to eat a food pellet (about 0·20 gm.) in the goal box and were then placed in a small cage for two or three minutes before being returned to the home cage. On non-rewarded trials the rat was removed from the apparatus 20 seconds after reaching the empty end box and spent the inter-trial interval of four minutes in the small cage used for this purpose.

All time scores were converted into logarithmic form before comparisons were made. In this way the occasional very high time scores are prevented from weighting the results too heavily. This is the usual procedure with this type of data and has been recommended on statistical grounds by Gaddum (1945).

III

Results

Let us test first the secondary hypothesis derived from Hull's theory of the drive stimulus, namely that the presence of the thirst stimulus during training trials will lead to its acquiring a habit loading with the running response. This would mean that, among those groups running thirsty or hungry plus thirsty on non-rewarded runs, those animals that had previously had their rewarded runs thirsty as well as hungry, would run faster. Among those run hungry only on non-rewarded runs, the animals previously thirsty would perform more slowly. Two components may contribute to the variance of response measures on non-rewarded trials: motivation during these non-rewarded trials and motivation during the previous rewarded trials. An analysis of the variance of these response measures will reveal whether both, either or none of these components make a significant contribution to it.

Table I sets out the mean log running time of each of the 35 rats on non-rewarded trials. The scores are divided according to the six experimental groups. In naming the groups a letter is used to designate motivation during rewarded runs (A = hungry plus thirsty, B = hungry only) and a number to indicate motivation during non-rewarded runs (1 = hunger only; 2 = thirsty only; 3 = hungry plus thirsty).

TABLE I

Mean log times on Non-rewarded Trials

	Groups						
Rats	*1A*	*2A*	*3A*	*1B*	*2B*	*3B*	*Total*
1	0·2135	0·3042	0·3228	0·2432	0·3605	0·3671	—
1	0·2059	0·3844	0·3060	0·3411	0·3099	0·2822	—
3	0·2407	0·3168	0·3140	0·1793	0·2635	0·3879	—
4	0·1722	0·3563	0·2124	0·2874	0·3680	0·3935	—
5	0·2375	0·3046	0·3172	0·2306	0·3291	0·3548	—
6	—	0·2971	0·3836	0·3219	0.1905	0·2974	—
Σ X	1·0698	1·9634	1.8560	1·6035	1·8215	2·0829	10·3971
Mean	0·2140	0·3272	0·3093	0·2673	0·3036	0·3472	—
Σ X²	0·2320	0·6486	0·6241	0·4469	0·5753	0·7352	3·2621

An analysis of variance performed on these values yields the following results :

Total sum of squares .. = 0·1731
Sum of squares between groups = 0·0626
Sum of squares within groups = 0·1105

TABLE II

Components	*Sum of sq.*	*d.f.*	*Variance*	*F*
Motiv. Reward runs ..	0·0040	1	0·0040	1·050
Motiv. Non-reward runs ..	0·0485	2	0·02425	6·365
Interaction	0·0101	2	0·00505	1·325
Within groups	0·1105	29	0·003898	—

The variance ratio for motivation during non-reward trials is significant at the 1 per cent. level. Neither the ratio for motivation during reward runs nor the interaction variance are in any way significant.

Thus it appears likely that the marked differences in the time scores of the groups observed on non-reward trials are entirely due to differences in motivation on these trials and are not affected by differences in motivation on previous rewarded trials.

When reaction latencies instead of running times are taken as response measures on non-rewarded runs, the analysis of variance yields exactly the same picture. The F value for the component, motivation during rewarded runs, is 1·917, while that for motivation during non-rewarded runs is 12·40. The latter is highly significant.

Finally, Bartlett's test for the homogeneity of variance was applied to the data. The result indicated that the assumption of homogeneity was fulfilled by the material.

We can now analyse the data further so as to fulfill the main purpose of the experiment, namely to compare various conditions of motivation under conditions of reward and non-reward so as to gain some information about "need interaction."

The response measures obtained on rewarded training trials are set out in Table III, which gives mean log times for each of the two groups (hungry plus thirsty and hungry only) on each of the ten days of training. As two runs were given on each day (except the first) the daily score for each rat was the mean of the two runs.

TABLE III

MEAN LOG TIMES ON REWARDED RUNS

	Group A (H. + T.)		*Group B (H. only)*	
Day	*React. Lat.*	*Run. Time*	*React. Lat.*	*Run. Time*
1 ..	1·06	1·02	1·06	1·00
2 ..	0·89	0·76	0·87	0·85
3 ..	0·46	0·60	0·67	0·74
4 ..	0·42	0·56	0·48	0·67
5 ..	0·40	0·49	0·29	0·53
6 ..	0.25	0·49	0·26	0·47
7 ..	0·14	0·43	0·19	0·44
8 ..	0·20	0·41	0·14	0·45
9 ..	0·27	0·39	0·12	0·40
10 ..	0·24	0·42	0·12	0·39

That the two groups are quite comparable at the beginning of training is shown by the fact that they start off at exactly the same mean response latency and that the difference between the mean running times on the first trial is quite insignificant. Now it will be observed that, whereas the mean response-latency values for the two groups cross and recross continually during the days of training, the mean running time values of group A (hungry + thirsty) are consistently below those of group B during the first half of the training series. In order to determine whether this trend was statistically significant the log running-time values for the 2nd, 3rd, 4th and 5th days of training were added for each rat. The mean of these total running-time values was calculated for each of the two experimental groups and the significance of the difference between them was determined by the t test. This roundabout procedure was necessary because each measured value is not an independent observation, the running times of any rat are likely to be correlated among themselves. The number of degrees of freedom, therefore, depends upon the number of rats not upon the number of observations.

The value of t obtained in this way was 3·115, which at 33 degrees of freedom is significant at the 1 per cent. level. The extra thirst drive in group A therefore leads to a significant increase in running speed on early learning trials, but does not seem to affect the response-latency measure.

The time scores obtained on non-rewarded trials were also converted to logarithmic form and the mean log value was calculated for each group on each of the six non-rewarded trials. Table IV gives these mean log values for the six groups.

TABLE IV

MEAN LOG TIMES ON NON-REWARDED TRIALS

Group	Resp. Measure	Trials					
		1	2	3	4	5	6
1A	Running time ..	0·308	0·389	0·541	0·429	0·494	0·735
	Latency	0·053	0·204	0·265	0·609	0·468	0·559
1B	Running time ..	0·457	0·553	0·586	0·924	0·782	0·796
	Latency	0·042	0·177	0·293	0·388	0·263	0·387
2A	Running time ..	0·595	0·620	1·255	1·256*	1·086	1·043*
	Latency	0·389	0·642	1·046	1·086*	0·892	1·160*
2B	Running time ..	0·508	0·998*	1·278*	1·040*	0·824	0·908*
	Latency	0·341	1·243*	1·010*	0·978*	0·791	1·586*
3A	Running time ..	0·724	0·814*	1·176	0·982*	1·043	1·056*
	Latency	0·367	0·757*	0·894	1·171*	0·912	1·072*
3B	Running time ..	0·504	1·232	1·178	1·224	1·352	1·293*
	Latency	0·304	0·693	1·145	1·100	0·956	1·651*

In the case of those mean values marked with an asterisk one or more animals in the group failed to leave the starting box of the runway within three minutes of

being placed in the starting box. This means that the mean performance figure for the groups affected in each case is based on less than six cases. This is unavoidable because, when the animal does not leave the starting box, it is obviously meaningless to speak of a reaction latency or a running speed. In effect the mean values obtained in this way are lower than if the experimenter had waited indefinitely until the rat finally responded. As these cases of no response within three minutes occurred only in the groups whose means were high in any case, the effect of neglecting the indeterminate scores would be to decrease the differences between groups and not to increase them.

The next step is to test the significance of the differences between the groups on the non-rewarded, test runs. Although there are six experimental groups there were only three different conditions of need on non-rewarded runs. The different treatment of each member of a pair of groups on the previous rewarded runs was shown in the previous analysis not to affect differences between performances on non-rewarded runs. Therefore we may combine the A and B sections of each of the three groups in calculating differences between them. Thus we have three groups to compare—1 (hungry only), 2—(thirsty only), 3—(hungry plus thirsty). For each group, the mean response time over the last five runs was calculated and the significance of the differences between them was assessed by the t test. It is necessary to leave the first of the non-rewarded runs out of account in this comparison, because any differences occurring here could not be due to the effect of non-reward which only enters the picture for the first time at the end of the trial when the time scores have been taken.

The following are the t values obtained in this way:—

Dif. 1 and 2, $t = 4{\cdot}898$, $P = 0{\cdot}0002$
Dif. 2 and 3, $t = 0{\cdot}291$, $P = 0.3848$
Dif. 1 and 3, $t = 4{\cdot}371$, $P = 0{\cdot}0005$

The pattern of results for running times and for latencies did not differ as it did on the rewarded runs; the above values have been calculated from the composite measure of total response time.

The existence of the differences is clear. As for their direction, a glance at Tables I and IV will show that group 1 is consistently faster than the other two groups. In other words, those animals which are only hungry but not thirsty on non-rewarded runs, i.e. which are running under "relevant" need alone, are faster than those animals which are running in the presence of thirst, the "irrelevant" need, as well as hunger. Thus the relationship between the groups run hungry only and those run thirsty plus hungry is the reverse on non-rewarded trials from what it is on rewarded trials. On rewarded trials the presence of thirst was seen to make for faster running, at least during the first few trials, but on non-rewarded trials the presence of thirst is seen to have quite the opposite effect, namely a slowing down of the response.

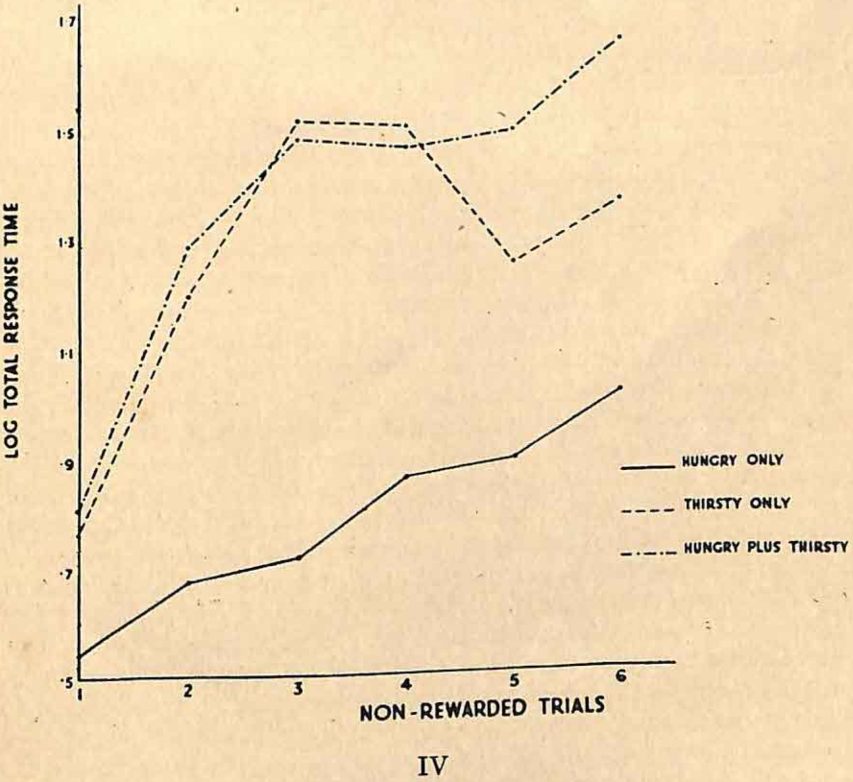
It is further interesting to note that the difference between the groups run hungry only and those run thirsty only shows a tendency in the same direction, namely a relatively slower performance in the case of the thirsty animals.

It may help to make these results clearer if they are presented graphically. Fig. 1 shows the mean total response time on each of the six non-rewarded trials.

It will be seen that the differences between groups appear very quickly. The groups 2 and 3 running in the presence of thirst reach a low level of responding within three trials and then remain more or less at that level; those animals which are running only in the presence of hunger approach this level much more gradually. The progressive slowing up in all groups is, of course, due to the fact that the trials were all non-rewarded or extinction trials.

It will be noticed that the differences between groups already appear on the very first non-rewarded trial, before the experience of an empty end-box could have exerted an effect on the animals. Does this mean that the condition of no reward is after all irrelevant for the differences between the groups? Such a conclusion would not be warranted. The differences between groups on the first trial are not statistically significant (the largest t value obtained for any pair of groups was 2·01), whereas on later trials these differences certainly do become significant. The differences on the very first non-rewarded trial seem to be due to the fact that the random splitting of the rats happened to result in a concentration of many of the fastest runners in group 1A and 1B. This appeared from an examination of the individual differences in response times on the last few rewarded trials. But this factor can hardly account for the differences between groups on subsequent non-rewarded trials, as these differences are considerably and consistently greater than the differences on the first non-rewarded trial. As a further check on the possible contribution of this factor to differences on non-rewarded runs the animals were divided into two approximately equal groups of slow and fast runners on the basis of their performance over the last few rewarded trials. The scores on the non-rewarded runs were then subjected to an analysis of variance using motivation (hungry, thirsty, or hungry plus thirsty) and previous slowness (estimated from the rewarded runs as above) as components. The result was an F value of 10·87 for the motivation component, which is significant at the 1 per cent. level. Thus this factor continues to contribute significantly to the differences between groups when the rats are divided on the basis of their previous slowness of responding. It is not likely, therefore, that the results obtained constitute merely a difference between "naturally" slow and fast runners, and we may proceed to treat the differences found between groups as being in fact due to the different treatment the groups received.

FIGURE I



IV

Discussion of Results

The evidence suggests that in the present experiment the "drive stimuli" of hunger and thirst did not acquire any habit loadings in the way Hull has postulated.

If they did, then the change-over from one set of conditions of motivation to a second set should have affected the variance of scores under the second set of conditions, which did not happen. Thus, according to the drive-stimulus theory the change-over from hunger (condition B) to thirst or to hunger plus thirst, for example, would have represented a certain shift along the stimulus-generalization gradient which was greater than the shift from hunger plus thirst (condition A) to these new conditions. Yet groups 2B and 3B did not differ significantly in their performance on non-rewarded runs from groups 2 A and 3A.

The view of drives as having internal stimuli as an essential element is one which is now so completely discredited as to need little further comment. It dates from a time when the animal was regarded as a system of buttons and wires and no conception of the dominant integrative action of the highest nervous activity had yet found its way into comparative psychology. Much of the evidence against the local stimulus theory of drive has been ably summarised by Morgan (1943) and need not be repeated here. It remains to deal with those cases of apparent discrimination between states like hunger and thirst which have been reported in the literature (e.g. Leeper, 1935; Hull, 1933). Those cases are in all probability due to an acquired discrimination between a full stomach (thirsty but not hungry) and an empty stomach (hungry but not thirsty), as has been shown very neatly by Heron (1949), who pumped the hungry animals' stomachs full of water and found that they responded as though thirsty and not hungry. Now while it is possible to identify an empty stomach with the hunger drive, it is hardly possible to identify a full stomach with the thirst drive. In the present experiment both the A and the B groups of animals ran with an empty stomach on rewarded runs, and so it is not surprising that no differential effect was noticeable when stomach conditions were changed. No effective discrimination between hunger alone and hunger plus thirst seems possible on this basis.

Turning now to the main body of results we may consider first of all the comparison between H and H + T groups on rewarded and non-rewarded runs. In the first case the hungry animals were slower, in the second case faster than the H + T animals. According to the theory which considers hunger and thirst as internal drives or pushes the interaction of such pushes is in terms of summation which occurs automatically as they have no specific direction and so cannot oppose one another. Moreover, as the push is one which originates outside the nervous system it has no direct connection with the external world. It can determine the organism's adaptive behaviour, but is not itself immediately determined by it; only by the fact that they lead eventually to need satiation do the ordinary activities of the organism affect its state of need. Accordingly, the interaction between drives does not in any way depend on the external situation at the time. This is clearly expressed by Hull (1943) and seems to be implicit in many statements made by certain psychotherapists. The present experiment does not support such a view, for it shows that in the rat the presence of thirst in addition to hunger acts differently, according to whether food is available in the situation or not.

As has been indicated earlier, it is believed that a specific explanation of these observed relationships lies to hand in the conceptual framework used by Pavlov. For the thesis of the effect of the external world on the internal physiological processes of the organism was undoubtedly the guiding principle in all Pavlov's research. This is expressed in the *conditional* reflex where a physiological process becomes contingent upon certain external conditions. The term essentially implies no more than this—it is so far from implying any doctrine of S–R bonds that Pavlov actually took sharp issue with those who sought to interpret his work in this way. (Pavlov, 1932.) Unfortunately, this had little or no effect on later misrepresentations.

Let us assume the existence of nervous centres for eating and drinking which are linked by a mutually inductive relationship so that the activity of the one inhibits the other. Let us also accept the overwhelming evidence gathered in Pavlov's laboratory on the link between these centres and stimuli associated with their activity, i.e. conditioned stimuli, accepting that these links imply an excitation of one by the other. And finally, let us make the well-known distinction between excitatory and inhibitory conditioned stimuli, the latter being associated with non-reinforcement, the former with reinforcement. Then in the present case we have the conditioned stimuli from the runway activating the food centre on rewarded runs. But after the first non-rewarded run these begin to exert an inhibitory effect and so tend to depress activity in the food centre. How does this affect the relation between the food centre and the drinking centre? On rewarded runs the conditioned stimuli associated with food will favour the food centre so that it inhibits the activity of the drinking centre and only food-directed behaviour results. Now Pavlov's experiments on the relationship of centres linked by bonds of mutual induction have shown the reciprocity of this relationship (Pavlov, 1927, Ch. 11). Thus the excitation of A leads to the inhibition of B and this in turns leads to an augmented excitation of A. This is what occurs in the present case when the food centre shows a higher level of excitation in the presence of the inhibited activity of the drinking centre than when acting alone; a fact which is manifested in the better running times of the H + T groups on rewarded runs. This higher level of excitation feeds into the motor centres so that it manifests itself in the intensity of the reaction, but not in its latency, because the latter depends on the establishment of certain links which is a process independent of the level of excitation within certain limits. Towards the end of the rewarded runs both groups are, of course, approaching their physiological limit of responding, so that the differences between them disappear.

On non-rewarded runs, on the other hand, the activity of the food centre becomes depressed and this means that the balance between it and the drinking centre now shifts in the latter's favour. The drinking centre escapes from its inhibition and in turn becomes the dominant factor. This means that the food centre and its connections undergo a secondary inhibition and the animals cease to perform those movements which have previously led to food. Instead they perform new exploratory movements in search of water. This is reflected in their poor time score as compared to animals which receive their extinction runs in the absence of thirst (compare groups 1 and 3), where this secondary inhibition does not occur. This factor appears both in reaction intensity and latency because the inhibition affects all previously established links.

One possible version of the theory which recognises only quantitative summation of needs may interpret the poorer performance of group 3 by suggesting that the higher absolute level of need in this group produced a disintegrative effect on the habits which sufficed to regulate a lower need level (French, 1941). That this is not so is indicated by the equally poor performance of group 2, which was thirsty only, and by another experiment which may be briefly quoted here. In this experiment a group of seven rats of the same strain was run under the same conditions as obtained in the present experiment, except that on the non-rewarded runs they were run at 46 hours food deprivation. They actually ran significantly *faster* than a group run at 23 hours hunger only. Thus it is not likely to be the higher absolute need level which is producing the slow running of group 3.

The observation that group 2 (thirsty only) ran just as slowly as group 3 throws further interesting light on the interrelation between the two centres. The fact that on the first non-rewarded trial group 2 is still performing quite well shows the efficacy

of the external stimuli in maintaining a level of activity in the food centre in spite of relative internal "satiation." This has been explored more fully in a previous study (Danziger, 1951). But once these external stimuli change their significance the dominance of the thirst centre is indubitably established. The similarity of the graphs for groups 2 and 3 in Fig. 1 indicates that the dominance relation of the centres is relatively independent of their internal activation, but is extraordinarily sensitive to the precise external conditions. Two non-reinforcements of the one apparently suffice to reverse the relationship between the centres under these conditions. The period of transition from the dominance of the one to that of the other is correspondingly short.

It remains to consider briefly some recent American experiments on "drive interaction" in the light of the present results. Kendler (1945) extinguished a bar pressing response previously reinforced by food under 22 hours hunger plus varying periods of water deprivation. He found that whereas resistance to extinction was a function of the strength of thirst up to 12 hours of water deprivation, there then occurred a sharp reversal of the relationship so that under conditions of 22 hours hunger + 22 hours thirst the resistance to extinction was actually less than when no thirst was present. A similar result was obtained by Siegel (1946). This is readily explained in terms of the mechanisms referred to in this discussion. While the excitation of the food centre is greater than that of the drinking centre the latter is inhibited and, by virtue of their reciprocal inductive relationship, increases the excitation of the former as in the first part of the present experiment. But when the relationship between the centres is reversed by increasing the internal activation of the thirst centre, the food centre becomes inhibited in its turn, and so, as in the second part of the present experiment, the performance on non-rewarded trials actually becomes worse.

An experiment by Webb (1949), which seemed to show a regular increase in resistance to extinction as a function of thirst up to 22 hours, seems to contradict these results. It is, however, open to a serious criticism. While Kendler used animals which were all at a determined and constant level of food deprivation, Webb used thirsty animals which had just been fed. Now the amount of food eaten by a thirsty rat varies inversely with its degree of thirst, so that Webb was probably measuring simply the effect of varying amounts of residual hunger on resistance to extinction. In a number of experiments by Estes (1949*a*, 1949*b*) the usual procedure which makes hunger the need that is rewarded was reversed so that the animals were first trained in a bar-pressing response with a water reward and then extinguished under conditions of hunger. Some responses to the bar were obtained under these latter conditions. As the training was carried out under the fairly severe conditions of 23 hours water deprivation, the intense excitation produced by reinforcements under these conditions may have produced an irradiation to the food centre so that a connection between it and the bar may have been formed. This possibility derives from the established empirical relationship between conditions of intense excitation and the wide spread of excitation under these conditions, as for example in an audiogenic seizure. The same mechanism has been suggested by Ischlondsky (1952) to account for the linking of extraneous stimuli with sexual excitement as in some sexual perversions. The precise level of excitation necessary for this mechanism to supervene under specific conditions is a matter for further empirical investigation. However, at all ordinary levels of excitation the relationship of mutual inhibition seems to govern the behaviour of the neural centres mediating tissue needs.

Conclusions

It would seem that a theory which stresses the extra-neural, humoral and internal

stimulus aspects of primary needs is not able to give a satisfactory account of need interaction as it manifests itself in behaviour. This theory, which is explicit in the writings of Hull and at least strongly suggested by certain psycho-analytic formulations, begins with a general fund of energy or libido which is supposed to provide the driving force without which the organism would be a lifeless structure. Accordingly, the interaction between needs is held to be (*a*) relatively independent of the momentary external situation and (*b*) to take place by quantitative summation or subtraction.

On the other hand, an increasing body of evidence from physiology and from the field of animal behaviour suggests that central neural processes play a dominant role in needs and that these processes are extraordinarily sensitive to external stimulation. Pavlov's work on the conditional activity of the food centre now assumes a new importance, for it provides the only clear and apparently valid theory of the interaction of need centres. The specific results of the present experiment could certainly not have been explained in any other way. The hypotheses of the mutual inductive relationship between these centres and of their activation or suppression by the appropriate external, conditioned, stimuli seem necessary to cover this field as it stands at present. One important consequence of this approach is that the rather metaphysical dichotomy between the driving and the driven part of the organism disappears.

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THE INFLUENCE OF ADAPTATION ON ABSOLUTE THRESHOLD MEASUREMENTS FOR OLFACTORY STIMULI

BY

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A group-test method of determining the mean absolute olfactory threshold to a test stimulus for a group of subjects is described. Probit analysis is employed to evaluate the results. This technique is utilized to measure the change in sensitivity to the test stimulus due to adaptation to the masking stimulus. When the values of the logarithm of the threshold concentration of the test stimulus are plotted against the logarithm of the concentration of the masking stimulus for a pair of odorous materials, a straight line is obtained. It is suggested that the gradient of this line may be used as a measure of the degree of community of odour-property betweeen the pair of substances.

I

Introduction

The aim of the present experiment was to devise a method of determining the absolute olfactory threshold for a test stimulus under conditions of adaptation to a masking stimulus, the ultimate aim of the investigation being the classification of odorous substances on the basis of their mutual interaction measured in terms of specific sensory adaptation.

A study of olfactory adaptation was made by Woodrow and Karpman (1917). They measured the time required for the sensation of odour to disappear when a steady stream of odorous vapour entered the subject's nose while he was breathing through his open mouth. They showed that this time decreased with increase in the physical intensity of the stimulus.

Komuro (1922) and Zwaardemaker (1925) carried out experiments in a chamber maintaining an odorous atmosphere, the masking stimulus, for the subject to inhale. A Zwaardemaker olfactometer containing the test stimulus was used within the chamber to determine the subject's threshold. Komuro investigated the change in sensitivity to the test stimulus with change in concentration of the masking stimulus in the case when the two stimuli were identical and the case when they were different. He found that the subject's sensitivity as measured by change in threshold decreased with increase in the physical concentration of the masking stimulus. Zwaardemaker determined the change in sensitivity to the test stimulus with prolonged application of the masking stimulus and found that the subject's sensitivity decreased steadily as the duration of the masking stimulation increased.

Elsberg (1935) measured the duration of the state of reduced sensitivity after stimulation had ceased. Using the stream injection technique for imposing the masking stimulus and the blast injection technique for determining threshold values, he was able to measure the duration of the state of reduced sensitivity under conditions of birhinal and monorhinal presentation of the masking stimulus. He showed that this duration depended on the physical intensity of the stimulus and the time during which it was applied. It was greater for bilateral than for unilateral stimulation.

Le Magnen (1947) described a method of measuring the change in the value of the absolute threshold to the test stimulus due to applying the masking stimulus for ten minutes. In this case the masking stimulus was inhaled through a mask and not streamed into the nose under external pressure.

Thus the methods of applying a masking stimulus and subsequently determining a threshold value for a test stimulus may be divided into two groups:—

(i) Those in which the masking stimulus is applied for a controlled time interval, after which the threshold measurement is made, e.g. Le Magnen's method.

(ii) Those in which the threshold measurement is carried out while the masking stimulus is being applied, e.g. Zwaardemaker's method.

In the light of the results obtained by earlier workers it is reasonable to assume that during a threshold determination which took as long as six minutes any results obtained by methods of type (i) would be invalidated by the steady increase in sensitivity during the test due to termination of the masking stimulation. In the case of methods of type (ii) the results would be vitiated by the gradual decrease in sensitivity throughout the test due to the continued masking stimulation. According to Elsberg's results the time taken to regain normal sensitivity depends upon the duration of the stimulation. If the masking stimulus is applied for only a short time, e.g. for the duration of one inhalation, then the state of reduced sensitivity would decay completely in 30 seconds. This result was utilized in the procedure developed for the present work.

II

Experimental Procedure

A group test, described below, which had been evolved during pilot investigations, was used to determine threshold values. This technique, which we shall call the sniff-bottle group test method, was selected for the present work on the basis of a comparative study made by Bursill (1951).

The testing was done in a light well-ventilated room, the subjects being seated at separate tables set well apart. Each subject was provided with a bottle containing the masking stimulus and a record sheet both of which he retained throughout the test. A series of bottles containing the test stimulus were presented to him in a prearranged random order. The same test bottles followed each other for all subjects, but each subject entered the cycle at a different point.

The bottles used for these tests were thoroughly cleaned and rendered odour-free before use. The procedure adopted for this was to wash the bottles in sulphuric acid and then to rinse them thoroughly in much water before placing them in the sterilizer where they were boiled for three-quarters of an hour on three consecutive days. The bottles remained in the sterilizer between the boilings and were not removed for use until they had cooled.

The bottles containing the masking stimulus were wide-mouthed glass-stoppered 16-oz. reagent bottles. A 100 ml. of a standard aqueous solution of the masking stimulus material was placed in each.

The test stimulus was presented in a range of concentrations. Twelve 8-oz. standard reagent bottles were each labelled with a letter taken at random from the alphabet. Each test bottle was covered by a paper jacket to prevent the subjects observing the effervescence of the more concentrated solutions on shaking. Nine of the bottles were loaded each with 50 ml. of water and these were used as "catch" experiment bottles ("Vexierversuchs- in each bottle being different. The remaining three bottles in the series were loaded with 50 ml. of water and these were used as a "catch" experiment bottles ("Vexierversuchs-flaschen") as a check against guessing and as an indication of the reliability of the test. The range of concentrations was approximately equally distributed on either side of the expected threshold concentration as determined in a pilot run.

Instructions to subjects

"There are twelve test bottles like the smaller one on your table, each containing an aqueous solution of the test material (–). In some of the bottles the concentration of the odorous material is high and you will easily be able to detect its presence, in others the concentration is much lower and you may not be able to detect the odour

at all. I want to find what is the lowest concentration you can detect immediately after taking a sniff from the bigger bottle on your table. The procedure of the test is as follows (demonstration) take one good sniff from the big bottle with your nose well into the neck of the bottle. Then exhale completely through your nose and at the same time replace the stopper in the big bottle and be ready to take a good sniff from the test bottle. When you have done this replace the stopper in the test bottle and shake well to saturate the air space in the bottle with odour ready for the next person to use it. Record on the sheet provided the letter on the test bottle and a plus if you detect the stimulus odour and a minus if you do not; if you cannot decide you may record a ±, but try to give a definite response. When you have done this, please pass the test bottle to your neighbour. Each of the test bottles is labelled with a letter taken at random from the alphabet. The bottles do not come to you in order of concentration. The alphabetical order of the letters is not the order of concentration or presentation. In order to familiarize you with the procedure I am going to circulate the strongest test solution so that each of you may have a trial run. Remember that this is the odour for which you will be looking in the other bottles. Only give a positive response when you detect this odour. None of the bottles will contain a stronger odour than this, so do not be afraid to take a good sniff. The big bottle will be the same throughout the test; it is only your response to the test bottle that you should record. The odour in in the big bottle is $\left\{\begin{matrix}\text{the same as}\\ \text{different from}\end{matrix}\right\}$ that in the test bottles. I will give you a signal when I want you to sniff from the big bottle, continue the test in your own time, take one sniff only from each bottle and only pause to exhale completely between the big bottle and the test bottle. In order to give your nose time to recover between each test there will be an interval of 30 seconds before I give you the signal to sniff from the big bottle again."

The instructions were given verbally and during the trials any subject disobeying the instructions was corrected. The subjects were encouraged to discuss the test when it was not in progress.

III

Treatment of Data

The record sheet of any subject who claimed to detect the stimulus in two out of the three no-stimulus bottles was discarded, the subject's responses being considered unreliable. The responses of each subject were not considered separately any further, but the results of the group as a whole were analysed together and a "social threshold" calculated. The number of positive responses to each bottle was summed over all subjects. The indefinite ± responses to a bottle were scored by dividing them in the ratio of the definite responses recorded for that bottle.

The concentrations employed in the test series may be represented by $x = 1, 2, 3, \ldots$. If λ_1 represents the concentration in ml/litre of the strongest test solution in the series and $x = 1$ for this solution, then the remaining concentrations in the series are given by

$$\log \lambda_x = \log \lambda_1 + (1 - x)\log 2 \text{ for } x = 2, 3, 4, \ldots\ldots 9$$

It was found that if the observed percentage positive response was plotted against concentration of the test stimulus on the x-scale a normal sigmoid curve was obtained. A statistical technique was employed to define the procedure of interpretation and facilitate the comparison of the results obtained from different tests. The statistical technique chosen was that of probit analysis, as developed by Finney (1947); this technique yields a threshold value and an estimate of the reliability of this statistic.

For the present work, the threshold is defined as being the concentration of the test stimulus corresponding to a 50 per cent. positive response as calculated by probit analysis.

IV

Results of Preliminary Experiments

To assess the consistency of the sniff-bottle method of threshold determination five separate groups of subjects were tested under the same condition. The test stimulus was acetone, no masking stimulus was used. The results are recorded in Table I and show that there is no significant difference between the threshold values obtained from the different groups.

To ascertain whether individual differences of inhaling from the masking stimulus bottle would appreciably affect our results, two ways of administering the masking stimulus were compared. One group of subjects was instructed to take one sniff from the masking stimulus bottle immediately before taking a sniff from the test stimulus bottle. The second group of subjects was instructed to take five sniffs from the masking stimulus bottle before taking a sniff from the test stimulus bottle. As will be seen from Table II there was no significant difference between the threshold values obtained by the two methods, and method one was adopted for the main experiment.

TABLE I

Threshold, m_x	*S.E.*m_x	n	*Threshold, moles/litre* $\times 10^{-3}$	*95% limits probability, moles/litre* $\times 10^{-3}$	
7·14	0·37	12	3·88	2·06	6·13
7·41	0.41	9	3·21	1·52	5·34
7·20	0·23	21	3·72	2·74	5·06
7·12	0·31	15	3·93	2·36	5·83
6·46	0·39	9	4·53	2·79	10·31

TABLE II

Conc. mask. stim. f_M	*No. sniffs mask. stim.*	n	*Threshold, moles/litre*	*95% limits probability, moles/litre*	
0·0327	1	30	0·0326	0·0244	0·0436
0·0327	5	34	0·0254	0·0195	0·0332
0·0817	1	33	0·0512	0·0434	0·0603
0·0817	5	30	0·0590	0·0521	0·0668

V

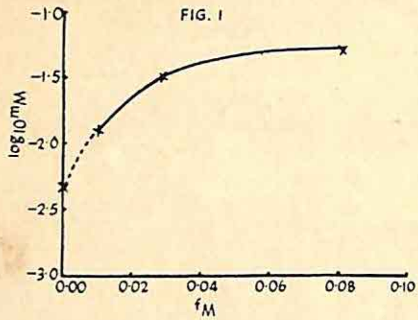
Results of the Main Experiment

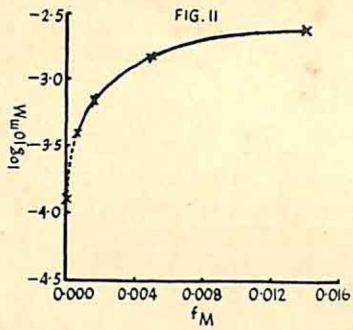
The aim of this experiment was to determine the manner in which the threshold value of the test stimulus varied with change in the physical concentration of the masking stimulus. The stimulus materials employed were isopropanol, dioxan and cyclopentanone. The experiment was divided into two parts:—

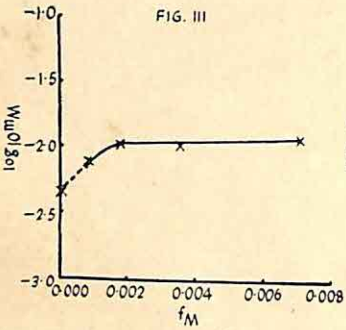
(*a*) Homogeneous pairs of substances, i.e. test and masking stimuli identical. Figs. I and II show the results obtained for isopropanol and cyclopentanone respectively.

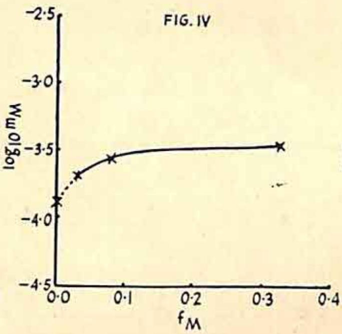
Using the experimental procedure outlined above the threshold value for the test stimulus m_M (concentrations expressed in moles/litre) corresponding to various concentrations of the masking stimulus f_M was determined. The figures were obtained by plotting the concentration of the masking stimulus as abscissa against the corresponding value of the logarithm to the base ten of the threshold concentration as ordinate. From the figures it is seen that the threshold to the test stimulus is raised when the concentration of the masking stimulus is increased in the range of concentration investigated. The rate of increase of the threshold value is seen to decrease with increase in the concentration of the masking stimulus.

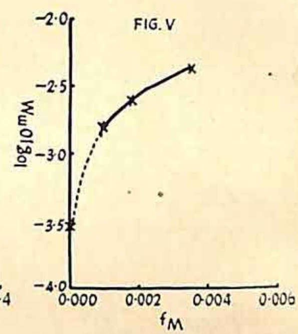
If instead of plotting the values of f_M as abscissa we use the $\log_{10} f_M$ the experimental values lie approximately on a straight line. Using the reciprocal of the

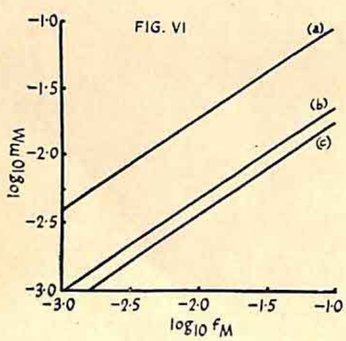












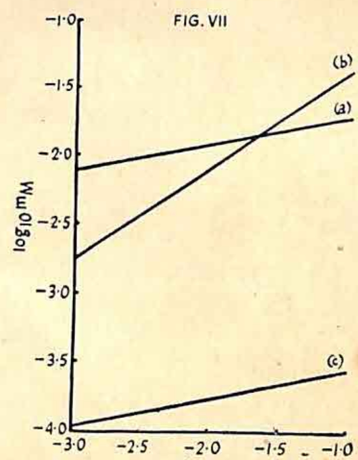


FIG. I.—Homogeneous pair, isopropanol. FIG. II.—Homogeneous pair, cyclopentanone.
FIG. III.—Heterogeneous pair, test stimulus isopropanol masking stimulus cyclopentanone.
FIG. IV.—Heterogeneous pair, test stimulus cyclopentanone masking stimulus isopropanol.
FIG. V.—Heterogeneous pair, test stimulus dioxan masking stimulus cyclopentanone.
FIG. VI.—Fitted regression line for homogeneous pairs.
(*a*) Isopropanol, (*b*) Dioxan, (*c*) Cyclopentanone.
FIG. VII.—Fitted regression lines for heterogeneous pairs.
(*a*) Masking stimulus cyclopentanone, test stimulus isopropanol.
(*b*) ,, ,, ,, ,, ,, dioxan.
(*c*) ,, ,, isopropanol ,, ,, cyclopentanone.

variance of each point as a weighting factor the method of least squares was employed to determine the best straight line for each series. It was found that in the case of the stimuli so far investigated the slope of the fitted lines was approximately $-0{\cdot}7$, these are shown in Fig. VI.

(*b*) Heterogeneous pairs of substances, i.e. test and masking stimuli different.

The results of these experiments have been treated in the same way as those of the homogeneous pairs. Fig. III shows the results obtained using cyclopentanone as the masking stimulus and isopropanol as the test stimulus, the results of the reciprocal combination are shown in Fig. IV. Using cyclopentanone as the masking stimulus and dioxan as the test stimulus Fig. V was obtained. In each of these figures the abscissa is f_M, the masking stimulus concentration, and the ordinate values are $\log_{10} m_M$, logarithm of the threshold concentration.

If as in the case of the homogeneous pairs we plot $\log_{10} m_M$ against $\log_{10} f_M$ we find we may fit a straight line to the experimental values. Table III shows the value obtained for the gradient of this fitted line in each of the cases investigated. As in the case of the homogeneous pairs the reciprocal of the variance of each point was used as its weighting factor and the line was fitted by the method of least squares.

The values shown in Table III may be contrasted with the results obtained for the homogeneous pairs. For the homogeneous pairs the gradient of the fitted line was found to be $-0{\cdot}7$ for each substance. If the threshold to the test stimulus was entirely independent of the concentration of the masking stimulus we should expect the gradient of the fitted line to be zero. For the substances investigated this condition is most closely approached by the pair cyclopentanone and isopropanol where the gradient of the line is $-0{\cdot}2$, the small decrease in sensitivity is clearly seen in Figs. III and IV. Fig. VII shows the same results as Figs. III-V, but $\log_{10} f_M$ is plotted against $\log_{10} m_M$ in this case.

TABLE III

Masking stimulus	*Test stimulus*	*Gradient*
Dioxan	Isopropanol	$-0{\cdot}5$
Isopropanol	Dioxan	$-1{\cdot}0$
Cyclopentanone	Isopropanol	$-0{\cdot}2$
Isopropanol	Cyclopentanone	$-0{\cdot}2$
Cyclopentanone	Dioxan	$-0{\cdot}7$
Dioxan	Cyclopentanone	$-0{\cdot}6$

VI

Discussion

In this work we have expressed the intensity of the odorous stimulus in terms of the concentration, in moles/litre, of the odorous material in aqueous solution, let us call this the M-scale of concentration. This measure of concentration has been used because from it we might—if we knew the vapour pressure and temperature relation—calculate the number of molecules of the odorous material present per c.c. of the vapour inhaled into the nose. We can only usefully talk about the intensity of

stimulation of the olfactory region in terms of the number of molecules of a particular species reaching the region in a given time. The appropriate scale of physical concentration which would give rise to a physiologically linear scale of stimulus concentration—the P-scale—is not yet known. But if the P-scale involves the physical concentration as a first power term only then transforming the results of these experiments from M-scale to P-scale, in the case where we have the logarithm of these values, will produce only a linear translation of the points on a graph, but will not alter their mutual relationships with one another. In the case of the results shown in Table III the gradient of the fitted lines will remain unaltered, only the intercept may be changed.

The gradient of the line obtained when we plot $\log_{10}m_M$ against $\log_{10}f_M$ may therefore be regarded as a characteristic of the pair of substances employed. We may use this gradient as a measure of the community of odour-property betweeen the masking and test stimulus materials. For homogeneous pairsof substances, i.e. test and masking stimuli identical, the "degree of community" of the two stimuli was found to be $-0{\cdot}7$, for the heterogeneous pairs investigated it varied from $-0{\cdot}2$ to $-1{\cdot}0$. By accumulating data in this form for an adequate number of odorous materials it is hoped that it may be possible to establish an hierarchical arrangement of the materials on the basis of the degree of community between each pair.

The choice of suitable stimulus materials is governed by certain general considerations: rather weak odours are preferred because of the greater accuracy with which the required solutions can be prepared and because this minimizes the effect of selective adsorption. To ensure reproducibility of experimental conditions only stable compounds of known constitution and high chemical purity may be used. It is very important that if there is a trigeminal component in the stimulus that the threshold concentration of this component should be much greater than the threshold concentration of the olfactory component so that no confusion can arise in the determination of the absolute olfactory threshold. These considerations rule out the use of essential oils and many other highly odorous complex organic compounds which have been extensively used in research in this field in the past. The choice is also governed by some considerations depending upon the experimental technique employed. The particular technique employed in this work imposes the further restrictions that the odorous substance should be freely soluble in water and stable in dilute aqueous solution, and introduces anxiety regarding the presence of water-soluble impurities.

The possible range of masking stimulus concentrations which may be investigated by the present technique is limited. For any pair of substances the experimental conditions will change when the masking stimulus is so dilute that the subject is unable to detect its presence. In Figs. I-V results are shown for threshold values corresponding to a zero concentration of the masking stimulus, i.e. using water as the masking stimulus. Interpolation between the result using zero concentration of the masking stimulus and the result of using a supraliminal concentration is shown as a dotted line, since this is the range of subliminal stimulation. In this region the subjects have a comparison with an absence of odour instead of with an odour. In this case any test bottle which stimulates the slightest suspicion will be recorded positive because the subjects will consider that they have in the masking stimulus bottle a standard of what is absence of odour. When the masking stimulus is detected then no such standard exists and there is greater uncertainty involved in judging the presence or absence of odour of the more dilute solutions. Therefore we may not employ a concentration of the masking stimulus which is so low that some subjects cannot detect the presence of the odour without effectively changing the conditions

of the experiment. The possible range of values of the masking stimulus is limited at high concentrations by the unpleasantness of taking a deep inhalation of a strong odour. If there is a trigeminal component this effect will be more pronounced, but in any case the subjects will attempt to decrease the size of the inhalation taken. Unless urged to follow the standard procedure the subjects will just take a small sniff at the neck of the bottle to avoid the unpleasantness of the strong odour. The results shown in Figs. VI and VII are calculated on the basis of results obtained in the optimum range of masking stimulus concentrations.

We may question the applicability of probit analysis for the determination of the threshold concentration. Probit analysis as developed by Finney was employed to determine the relative toxicity of various insecticides. In the experimental design used samples of a population of insects were exposed to different concentrations of the insecticide—one sample of insects for each concentration—the percentage of moribund insects was then found for each dose. The response of the insects at one concentration of the insecticide is therefore completely independent of the response at any other concentration because each individual insect is used once only. In the odour threshold calculations each subject is tested at all the concentrations, it is as though we were resuscitating the insects after one dose and giving them another and so on. This modifies the results obtained in two ways: firstly there is the effect of the experience gained by the subject during the course of the test, and secondly intrapersonal variations will be introduced. The first effect is randomized by keeping the bottles in the test series in the same order with respect to one another but ensuring that each subject enters the cycle of test bottles at a different point. The effect of the intrapersonal variations will tend to increase the variance of the calculated threshold. This will mean that the estimated variance of the threshold will be slightly smaller than the true value.

This paper is a preliminary report of the work which is still in progress. Further work will employ the technique described here to determine the mutual relationships between other stimulus materials.

VII

Summary

A simple group test method of determining the absolute olfactory threshold for a test stimulus is described. Probit analysis is used to calculate the threshold concentration corresponding to 50 per cent. positive responses.

This method of threshold measurement is used to determine the change in sensitivity to the test stimulus due to different concentrations of the masking stimulus.

When the results for a pair of stimuli are plotted on a graph whose axes represent the $\log_{10} f_M$ ($\log_{10}$ masking stimulus concentration) and $\log_{10} m_M$ ($\log_{10}$ threshold concentration) the experimental points lie on a straight line.

In the three cases examined where the test and masking stimuli are identical—homogeneous pairs—the slope of this line is $-0{\cdot}7$. For the six heterogeneous pairs investigated, i.e. test and masking stimuli different, the slope of the line varies from $-0{\cdot}2$ to $-1{\cdot}0$. This slope may be considered as a measure of a degree of community of odour-property between the pair of substances.

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THE RELATION OF AN ENVIRONMENTAL VARIABLE TO PERFORMANCE IN A PROLONGED VISUAL TASK

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In a previous paper (Fraser, 1950) on the relation between angle of display and performance in a prolonged visual task (Mackworth's Clock Test), it was suggested that the presence or absence of the experimenter might affect performance on vigilance tasks. In Mackworth's original experiments (Mackworth, 1950) the subject was in a closed cabinet by himself, and as far as possible all external noise and stimuli were excluded. In the angle of display experiment, the writer sat in the same room as the subject, out of his sight, and there was no particular attempt to exclude the ordinary external noises, etc. It seemed worth while to follow up the previous experiment with another to test the hypothesis that the presence or absence of the experimenter might influence performance in such a task. A new apparatus was devised for this purpose, which possesses some advantages over the Clock Test.

Apparatus

The apparatus consisted essentially of a simple projector system, a screen and a 35 mm. cine camera. The projector was fitted with a motor which drove 16 mm. cine film continuously through a gate without any claw system. Each frame of the film was perforated with a circular hole, cut by means of a leather punch. This moving display was projected on to the screen, which was of opal glass, and was placed between the subject and the projector. The subject was seated at a table on the edge of which was mounted a cine camera by means of a firm table clamp. The cine camera was fitted with a single picture device which was operated by winding a crank through one revolution.

The holes cut in the film were of 2 mm. diameter, one hole to each frame, but at long intervals chosen at random, a hole of larger diameter was cut. The task of the subject was to watch out for these larger holes, and to turn the crank through one revolution whenever he saw one. This photographed the display on Plus-X film, and constituted a simple and foolproof method of recording.

Procedure

For the first experiment, a size of hole just over 3 mm. in diameter was chosen. Twenty of these holes were interspersed at random throughout a series of 2 mm. holes, arranged so that 10 of the large stimuli occurred in the first half hour and 10 in the second half hour. Eighteen subjects (Naval Ratings) were tested for one hour:—

(*a*) alone in the experimental room, and
(*b*) with the experimenter present, but out of sight of the subject.

The order was randomized. No attempt was made to exclude all external noises in this experiment, in either of the two conditions; the presence or absence of the experimenter was the only variable.

Results

The table shows the number of errors made in each condition, and gives a value for t of 3·166. With 17 degrees of freedom this is significant at better than the 1 per cent. level, although the confidence limits for the mean value are high.

An examination of the raw data shows that the burden of this significance rests on seven cases, and that in the other eleven cases, the difference is zero or one in either direction. It is possible to split the group into a "good" section and a "poor"

TABLE I

ERRORS (MISSED SIGNALS) UNDER DIFFERENT CONDITIONS

	Total	*Mean*	"*t*"	*P*
Experimenter absent	39	2·17	—	—
Experimenter present	16	0·89	—	—
Difference	23	1·28	3·155	0·01

section, in fact. It is questionable how far it is legitimate to do this, but the results are consistent with the possibility that the task is not very difficult for the "good" subject, but is rather wearing for the poor subject. The concept of what might be called a "fatigable dimension" along which subjects may be placed, is suggested by the work of Broadbent (1950, 1951). We might expect a significantly positive result in an experiment such as the present one to depend on the composition of the sample; it might not appear in a small sample, or in a selected one.

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MISCELLANEA

A MULTI-CHANNEL PRINTING CHRONOGRAPH

BY

R. L. GREGORY

(*From the Medical Research Council Applied Psychology Unit, Cambridge*)

The Problem.

Experimental psychologists have always been concerned with the recording and timing of events. The most commonly used recording device is the Polygraph, which is a multi-channel development of the Astronomer's Chronograph. For its original use of recording star transits, the Chronograph was convenient and simple, but when the psychologist extends its use to measure, for example, multiple-choice reaction times, he uses several channels for comparatively long periods, which results in yards of record from which perhaps several hundred measurements have to be taken to derive the times of the recorded events. This limits the number of variables which it is practical to consider. Further, mistakes are apt to arise when reading the records through confusing the channels. It is also expensive in paper to operate at high speeds, and is not reliable since the pens are apt to become blocked.

The device to be described avoids these objections, but can be built by a mechanic without knowledge of electronics. The use of valves is entirely avoided.

The recorder has the following characteristics. There are 29 separate channels. Each channel is identified by a letter of the alphabet, or other symbol, which is printed upon the arrival of a signal on 2½-inch record paper. Time is recorded in printed numerals which may be read directly from the record. The recording paper is stationary except when a signal arrives, when, after printing, it moves 3/16ths inch to allow the next signals to be printed.

Design.

The main component of the recorder is the "Palantype" shorthand machine. This is a manually operated machine similar to a portable typewriter, but differing in essential respects. In particular, any number of its 29 keys may be depressed together; they will then all print simultaneously, the characters being ranged across the paper in a line. There is no carriage as in a typewriter; when the keys are released the paper tape is fed on so that the next line can be printed below the first.

The "Palantype" is ideal for the printing unit. Since any number of the keys can be pressed simultaneously a simple circuit can be used; if a normal typewriter were used it would be necessary to provide storage so that signals arriving together would be printed in sequence. This would be inconvenient and is avoided by arranging each channel to be independent throughout.

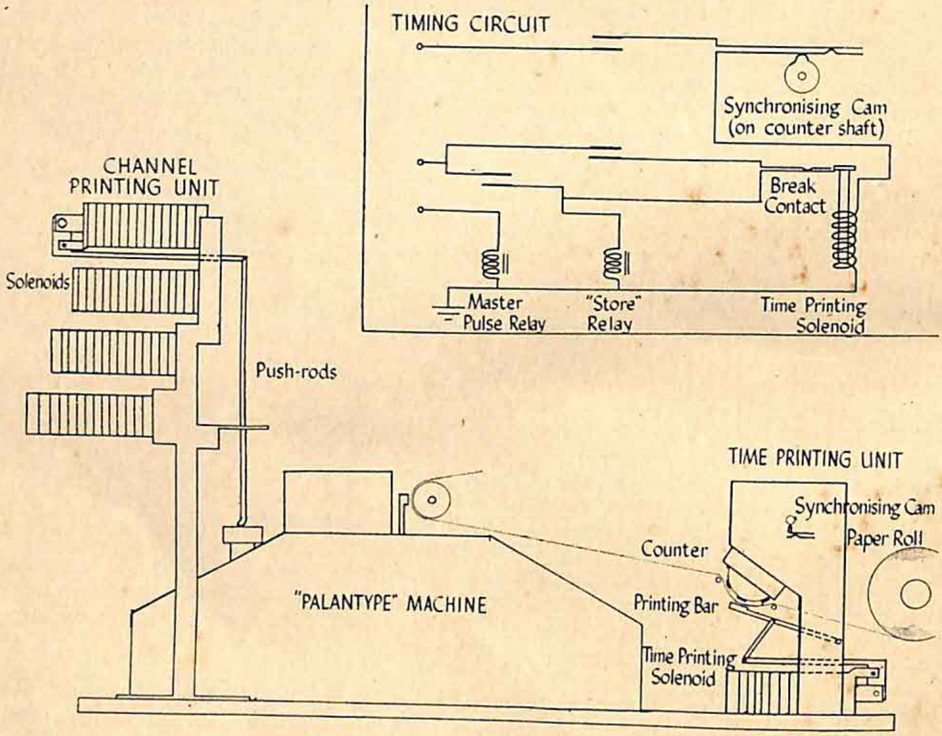
Time is also printed on the record paper; this is done with a printing counter driven at constant speed and made to print whenever a signal is received. The four numerals which are printed give a direct record of the time of each recorded event on any of the 29 channels to the nearest 0·1 second. Carter, *et al.* (1951), used a similar shorthand machine as a recorder. They use it manually, with a constant speed paper drive and a time-marker as in the Polygraph. Welford (1951), using an electronic circuit and punched tape, codes the time to avoid continuous paper drive.

Construction.

(1) *The Channel Recorder.* The "Palantype" machine—which records the channels though not the time of each occurrence—need not itself be modified. It is only necessary to screw it down to a baseboard. Above the keyboard 29 solenoids are mounted and so arranged that each will press a key when energized. (The solenoids used are 1 inch in diameter; they are mounted over the keys, in four rows, by bolting to a framework. This is made from strip brass; the strips supporting the solenoids are bent to conform to the plan of the keyboard, which is V-shaped. The construction is shown in the figure. The solenoids are obtained from ex-Government Stores and are known as "Cantilever Solenoids, 24 volt.") The levers with which they are furnished are used to actuate vertically mounted push rods which rest on the "Palantype" keys.

It is not sufficient simply to energize the printing solenoids from the incoming signals to be recorded; they must be energized with standard length pulses of about 0·01 second duration. There are a number of possible ways of providing such pulses—the method adopted was suggested by Dr. Alfred Leonard, and has proved simple and reliable. Make-before-break relays are used (e.g. ex-Government, Ref. No. P. 27442 or P. 27710). One of these relays is placed in each solenoid circuit, the two sets of relay contacts being wired in series. Each relay is energized by signals on its channel; as it closes, a pulse is produced which energizes its printing solenoid for the desired 0·01 second. This pulse length is adjusted for each channel with a 1,000 Ω pre-set resistance in each circuit, and also with a master control in a metered 12-volt supply. (This supply need not be stabilized, but it should have at least a 4 amp. rating to prevent a serious drop in voltage when several signals are received together; a voltage tolerance of 20 per cent. is permissible.)

In addition to providing pulses for the printing solenoids, the make-before-break relays also serve to isolate the current required for the printing solenoids (which are run at 40 volts) from the external recording circuit.



(2) *Time Recorder.* This is built round a printing counter (series 428, supplied by English Numbering Machines, Ltd.). It has type-face numerals and is designed to be driven continuously; it is geared so that one revolution of the drive shaft shifts the first counter wheel one numeral.

To record time to the nearest 0·1 second the counter is driven at 600 revs./minute. It is mounted immediately behind the "Palantype" and arranged to print on the underside of the record paper; this is drawn over the counter by the "Palantype's" paper feed whenever a signal is received.

A printed record of the counter is obtained by arranging for a printing bar to strike the paper on to the type wheels; a ½-inch typewriter ribbon between the counter and the paper enables a reasonably clear impression to be obtained. The times are printed on the back of the record at a constant distance from their corresponding channel records; these are related with the aid of two slots in a simple reading desk, the times being read from underneath with a mirror mounted under the desk looking through one of the slots. To allow the numerals to be read directly in the mirror the type-face used is not inverted as is usual.

Before this system will work, certain precautions must be taken. The solenoid operating the printing bar must be supplied with a 0·01 second pulse whenever a signal is received on any of the recorder channels, and this pulse must be synchronized with the counter so that the striker only prints when a numeral on the high speed counter wheel is opposite the striker, otherwise this figure will be illegible. (The other figures look after themselves since they shift intermittently, but the first wheel is driven continuously from the drive shaft.) The solution adopted is to feed the printing bar's solenoid through a contact which is closed once, by a cam for 0·01 second, for each revolution of the drive shaft. This allows the mechanism to print only when a numeral is opposite the striker. But a further problem remains: if a signal arrives when the synchronizing contact is open, it may be missed, but this must not happen. The problem is solved by storing the information that a signal has arrived by making it energize a relay with a holding-down circuit. This remains energized and the solenoid is energized when the synchronizing contact closes, which will be within 0·1 second after the signal arrives. When the time is printed, the holding-down circuit is broken so that the striker cannot print again until another signal arrives. If more than one signal arrives within the minimum discriminating time of 0·1 second the time will, of course, only be printed once; the characters denoting the channels on which the signals were received will lie in line across the paper and the time printed will apply to them all. A discontinuous counter drive used with this circuit would have the advantage that the synchronizing would be less critical.

This method of recording time could be used independently of the rest of the recorder. It would then consitute a printing chronograph giving a time record which can be read directly. The rest of the recorder might be regarded as a means of extending the capacity of the printing chronograph from 1 to 29 channels.

A Portable Recorder.

It is hoped to develop a portable recorder. In place of the shaft-driven counter a ratchet counter will be used; the intermittent drive gives a better printing—dead-time ratio, and allows a clock-controlled solenoid to be used for driving the counter. This is compact and can be at least as accurate as a normal stop-watch. The number of channels will be reduced to about six; they will be printed with plungers mounted close to the counter and will be arranged so that they can be worked manually, with buttons, or from solenoids.

The Portable Recorder is to be made commercially available by: Nucleonic and Radiological Developments, Ltd., 22, Marshgate Lane, London, E.15.

Summary of Performance Characteristics.

1. It records time to the nearest 0·1 second on 29 independent channels.
2. The channels are identified by printed characters.
3. Time is recorded in numerals and read directly.
4. The record paper is stationary except when a signal is recorded when it moves on 3/16ths inch.
5. The minimum permissible duration of input signal is 0·03 second.
6. The maximum repetition rate on each channel is five signals per second.

Advice and encouragement were given by Dr. N. H. Mackworth, Mr. N. Welford and Dr. A. Leonard, of the Cambridge Psychological Laboratory. The machine was originally built for an experiment on submarine escape, undertaken for the Royal Naval Physiological Laboratory under the direction of Dr. S. J. Taylor. The "Palantype" was supplied by "The Palantype College," 229–231, High Holborn, W.C.1, with funds provided by the Medical Research Council. The counter was supplied by "English Numbering Machines, Ltd.," Queensway, Ponders End.

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A NEW RAT-PELLET FEEDING MACHINE

BY

H. M. B. HURWITZ

(*From the Animal Laboratory, Psychology Department, Birkbeck College, London*)

THERE have been several attempts to develop a feeding-machine for use in experiments on operant-response learning in the rat. Skinner (1938) who initiated research on lever-pressing behaviour as an instrumental response, and designed the prototype apparatus for this purpose, reports briefly on the machine's construction and a few other descriptions in greater detail have been published (e.g. Frick *et al.*, 1948; Mowrer & Miller, 1942). These machines have two main disadvantages.

Firstly, if the machine relies on a gravity feed, the use of uniform pellets is necessary in order to ensure delivery of one and not more than one pellet per lever-depression; uneven pellets are likely to jam the delivery chutes, thereby setting the apparatus out of action. In practice this means that experimenters tend to take great care in selecting the pellets to be fed through the machine, and often prefer to manufacture their own—a time consuming and ennervating task when catering for large groups.

Secondly, it is well known that the sharp click given by the machines in current use acts as a discriminatory, reinforcing, stimulus. It is difficult to eliminate this click or, alternatively, to maintain it during unreinforced as well as reinforced responses. Consequently some experimenters (Mowrer & Jones, 1945) have resorted to manual feeding in studies on partial reinforcement where control of secondary reinforcers is essential.

The machine to be described, which is now being used in an experiment on reaction chaining under varying conditions of reinforcement, consists of three basic units: a frame to support the mechanism and feeding trough, a rotary relay (post-office uni-selector) and a disc. The selector is mounted on a frame so that the spindle stands in a vertical position. A polished 3/32 in. plastic sheet, with a central hole for the protruding selector spindle, is screwed on to the brackets supporting the selector. A $\frac{1}{4}$-inch thick disc into which as many holes have been drilled as there are teeth on the gear wheel in the selector, fits tightly on to the spindle. As the disc is rotated, the food pellets, which are placed into the holes of the disc, drop through a small aperture in the plastic sheet and along a chute to the food trough, which has been mounted on the outer wall of the frame. In order to reduce the noise of its operation, the complete selector unit is permanently immersed in medium chassis grease in a glass container. By increasing the resistance jerky operation is prevented and the pellets remain in the small disc holes until released through the chute. A condenser connected across the input terminals eliminates sparking.

This machine has two further advantages: In order to increase or decrease the amount of reinforcement, the size of the pellets fed into the holes can be altered without affecting the operating of the machine; and secondly, the holes may be filled or left empty as required by a reinforcement schedule.

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BOOK REVIEWS

Über Aufbau und Wandlungen der Wahrnehmungswelt. By Ivo Kohler. Österreichische Akad. d. Wissensch. Sitzunsberichte, 227. Band 1. Abh. Vienna, Rohrer, 1951.

Asymmetries of the Visual Space. By Martti Takala. Ann. Acad. Scient. Fennicae. B, **72**, 2. Helsinki, 1951.

Orientation in the Present Space. By C. I. Sandström. Stockholm: Almqvist & Wiksell, 1951.

The authors of the three monographs cited above have in common a lively interest in visual problems, an experimental approach to their solution, and a refreshing freedom from doctrinaire theoretical constraints. One may hope that they are representative of a new generation engaged in rekindling the grand, if somewhat forlorn, traditions of European experimental psychology.

Dr. Kohler, whose preliminary communication to the 12th International Congress of Psychology at Edinburgh aroused lively interest, has now put his material together as a succinct and well-written monograph. His work is essentially a repetition and extension of the famous Stratton experiment on vision without inversion of the retinal stage, but using a wider variety of distorting media and paying greater attention to the after-effects of visual disorientation. Comparable experiments with "bi-tinted" spectacles are also described. Although no radically new line of explanation is proposed, Dr. Kohler's acute theoretical comments (especially those bearing on the central origin of visual after-effects) deserve careful scrutiny.

Dr. Takala's thesis embodies a report of some 80 simple, though well-conceived, experiments bearing on the notion of spatial anisotropy in the Gestalt sense. In view of Joynson's expert review in the *Brit. J. Psychol.*, 1952, **43**, 236–7, it is unnecessary to say more here, beyond commending the work in a general way to students of visual perception.

Dr. Sandström's study, though built around a very simple experiment, is somewhat wider in scope and intention. Impressed by the insecurity of autokinetic localization, he developed an interesting series of eye-hand co-ordination tests designed to study the accuracy of tactual localization of a luminous point in a dark room. The findings, which are reported in full, lead Dr. Sandström into an extended discussion of the body image and kindred neurological notions less familiar than they might be to experimental psychologists. Although rather a lot of theory is based on a slender foundation, Dr. Sandström's attempt to link up the special problems of visual localization with the broader issues of orientation should be capable of extensive development.

These three studies will be very generally welcomed by experimental psychologists in Great Britain and should provide a real stimulus to further work on the intractable problems of space perception. It is to be hoped that some attempt will now be made to link the work up with parallel investigations proceeding in this country and in America.

O. L. Z.

Design for a Brain. By W. Ross Ashby. London: Chapman & Hall, 1952. Pp. x + 260. 36s.

Dr. Ashby has spent some years in trying to develop a conceptual system appropriate to the analysis of adaptive behaviour. In this book he explains the line of his thinking and the point to which it has so far led him. Briefly, his approach is as follows. Beginning, as one obviously must, with the organism and its environment considered as a single closed system, he divides its behaviour into domains of approximately continuous variation, separated by sudden jumps or "breaks." The analogy is with a simple machine which obeys certain laws within a given working range, but if forced beyond that range undergoes a relatively abrupt change in its laws of a kind which may be called a "break," though it need not amount to a breakdown.

It is supposed that a living organism is distinguished by the possession of a large number of possible domains or "fields." If, in conjunction with a given environment, it is unstable in an existing field, its state will diverge from equilibrium until a break occurs and a new field is set up. If this too is unstable, a second break will occur, and so on until a stable field is reached, which will end the process for the time being. In this way the author attempts to establish trial-and-error learning on a rigorous logical foundation; from this, he argues, all other forms of learning grow. In discussing how they may do so, he considers some of the conditions necessary to minimise the risk of a catastrophic

instability—how, in short, an organism can learn efficiently, yet avoid the proverbial danger of "a little learning."

The description of the system in terms of "fields" and "breaks" is just one way of decomposing the statement, "the system is stable." It is a formal and generalized development of what might be called normal practice in thinking mathematically about reality; perhaps it is an epistemological—and therefore a psychological—necessity in the long run; if so, Dr. Ashby deserves credit for reaching what he would call a "terminal field" in this respect. Certainly the book should be judged rather as an attempt to find a fitting and adequate language than as a specific message in that language. The experimentalist, for example, need not expect to be told what to do next. But many psychological theorists might be the better for a course of Dr. Ashby's axioms and definitions.

The mathematical arguments have been wisely relegated to an appendix, leaving the main body of the book clearly written, fully illustrated, and well seasoned with farmyard and laboratory examples. Unfortunately, however, there is a good deal of padding in both parts, and much of the main text seems unnecessarily elementary. W. E. H.

Proceedings of the Experimental Psychology Group, 1952

31ST MARCH–1ST APRIL, 1952.—Extended Meeting at Oxford. *1st Session:* "The Effects of Social Class on the Attitudes and Behaviour of Adolescents," by H. Himmelweit. *2nd Session:* "Selective Retention in Amnestic States," by M. Williams (by invitation). "Some Notes on the History of Reaction-Time Experiments," by A. Leonard (by invitation). *3rd Session:* "Transposition Behaviour in Young Children," by I. M. L. Hunter (by invitation).

10TH–11TH JULY, 1952.—Extended Meeting at Cambridge. *1st Session:* "The Suppression Theory of Binocular Vision," by H. Asher (by invitation). *2nd Session:* "Studies of the Feeding Behaviour of Animals," by D. E. Tribe and J. G. Gordon (by invitation). Demonstration of Classroom Apparatus, by members of the Group. *3rd Session:* Symposium on "The Design of Classroom Apparatus," by A. Summerfield, G. C. Grindley, W. E. Hick and R. C. Oldfield. "The Psychology of the Frog's Retina," by H. Barlow (by invitation). *4th Session:* Short Communications by members of the Cambridge Psychological Laboratory—E. R. F. W. Crossman, R. L. Gregory and J. Szafran (all by invitation). "Simultaneous Vision and Audition," by G. H. Mowbray.

11TH OCTOBER, 1952.—Meeting at Birkbeck College, London. *1st Session:* Short Communications by members of the Department of Psychology, Birkbeck College—W. Lightfoot and F. Heller (both by invitation). *2nd Session:* "Some Psycho-physiological Relationships in Organizing and Controlling a Skilled Response," by C. B. Gibbs (by invitation). "Acquisition and Extinction of Behaviour Sequences," by H. M. B. Hurwitz (by invitation).

10TH JANUARY, 1953.—6th Annual General Meeting, at University College, London. *1st Session:* "On the Study of Perception," by C. H. Graham (by invitation). *2nd Session:* "Two Channel Listening," by C. Poulton.

The following Officers and Committee were elected for the year 1953:—

President:	Miss M. D. Vernon (Reading).
Editor:	Dr. D. Russell Davis (Cambridge).
Committee:	Dr. A. D. Harris (Cambridge).
	Dr. W. E. Hick (Cambridge).
	Mr. R. B. Joynson (Nottingham).
	Mr. H. Kay (Oxford).

At the first meeting of the new committee Dr. A. D. Harris was reappointed Hon. Secretary, and Dr. W. E. Hick, Hon. Treasurer. Mr. A. R. Jonckheere, B.Sc. (University College, London) was co-opted to the Committee.

New Members.—Dr. H. Barlow (Physiological Laboratory, Cambridge), Mr. D. E. Broadbent (Cambridge), Mr. P. H. R. James (University College, London), and Mr. J. Szafran (Cambridge) were elected members of the Group. The resignation of Dr. H. J. Eysenck (Institute of Psychiatry, London) was accepted.

Visiting Foreign Members.—Professor C. H. Graham (Office of the Assistant Naval Attache for Research, U.S. Embassy), Dr. S. Koch (University College, London), and Mr. J. von Wright (Institute of Experimental Psychology, Banbury Road, Oxford). Dr. H. Imus resigned on his return to the United States.

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ENTROPY AND CHOICE TIME:

THE EFFECT OF FREQUENCY UNBALANCE ON CHOICE-RESPONSE

BY

E. R. F. W. CROSSMAN

(*From the Medical Research Council Applied Psychology Unit, Cambridge*)

A human subject making a sequence of choice-responses is considered as a channel transmitting information. Earlier work suggests that the rate of transmission is limited, and so that response time is proportional to the "entropy" of the source of signals. Entropy is reduced by unbalance in the relative frequency of the possible signals according to the formula, sum ($p \log p$). Unbalance should therefore reduce average response time This prediction is tested in a card-sorting task. The subjects sorted playing-cards into classes in various ways; times taken were proportional to calculated entropy-per-card. Departures from the expected results occurred and were found to be due to differences in perceptual difficulty of discriminations. Some incidental results are mentioned.

I

Introduction

Previous work on multiple-choice reaction-times (Hick, 1952) has shown that the classical data fit well into the theoretical framework of information theory. This study is intended to extend and strengthen the evidence on which the use of the theory is based, by testing out a prediction which derives from it. Briefly, this is that, when a subject responds to a sequence of signals all of which belong to a known set but some of which occur more frequently than others, his average response-time will be proportional to the average information per signal. This follows from the hypothesis that the subject deals with information at a constant rate.

The original evidence that the information measure was the appropriate one to use for interpreting choice-reaction times was simply that the logarithmic function occurs in both. This in itself is not strong, since logarithmic relations occur rather often in biological measurement. The case became much stronger with Hick's finding that the reduction in response-time when errors are permitted obeyed the same law. Thus, from among the quantities entering into Shannon's expression for "Rate of Transmission" (see later), two have been studied: firstly, "n," the number of possible signals from which the actual one is chosen; and, secondly, the distribution of errors in the responses. In the present study, relative frequency of the signals for a constant total number is chosen as the independent variable. The results further justify our belief in the rightness of our theoretical framework.

II

Theory

A brief statement of the parts of Shannon's theory which bear on this work will be given here for convenience. The full statement can be found in his book

and elsewhere (Shannon, 1949). There are two distinct parts of the theory. The first deals with methods of describing signals, and the second with what happens in transmission.

Mathematical Description of Messages (Discrete Case)

A "message" consists of a sequence of "signals" chosen by a source from a set of n. Let signal i have probability p_i as seen by the receiver, and let successive signals be statistically independent. We define

$$\text{Information in signal } i = \text{I} = -\log p_i \qquad (1)$$

$$\text{Average information in the message per signal} = \text{H} = -\sum_{i=1}^{n} p_i \log p_i \qquad (2)$$

The latter quantity we call *entropy* of the source. It is measured in "bits" (when $p_1 = p_2 = 0{\cdot}5$, $\text{H} = 1$ bit). Entropy so defined is non-negative, has no upper bound as n increases, and is additive as between two independent sources. If the p_i are varied keeping n constant and $\sum_{i=1}^{n} p_i = 1$, it can be shown that H has a maximum when all the p_i are equal, i.e., when $p_1 = p_2 = \ldots p_n = \frac{1}{n}$. Any departure from equality of the p_i entails a reduction of H, and it is this reduction which forms the subject of this paper. Figure 1 shows typical sequences from a four-letter message, and the effect of frequency unbalance on information content. Besides reducing the entropy, unbalance also introduces unevenness in the flow of information, which can conveniently be expressed as a peak-to-mean ratio

$$\text{Q} = \frac{\text{Maximum information in any one signal}}{\text{H}} \qquad (3)$$

Transmission

Messages are only of interest as conveying intelligence from one point to another or governing some operation. Any such process can be thought of as transmission through a channel, and all channels have inherent limitations which restrict their efficiency, speed-limit and occurrence of error being the main ones. Shannon defines rate of transmission, R (see Figure 2) as

$$R = H(x) - H_y(x) \qquad (4)$$

where $H(x)$ = entropy of the source (per signal or per second)
$H_y(x)$ = entropy due to errors in transmission.

In ideal transmission, with no errors, $H_y(x) = 0$ and $R = H(x)$. To take an example, a man reads over a noisy telephone line a sequence of words taken from a small vocabulary and another man listens. The source entropy $H(x)$ is determined by the size of the vocabulary and the way in which words are chosen by the sender. Let the choices be made independently, with equal frequency, from a set of n possibles, at W words per second; then the source entropy $H(x) = \log n$ bits per word, or $W \log n$ bits per second. The listener may hear well, and make no mistakes.

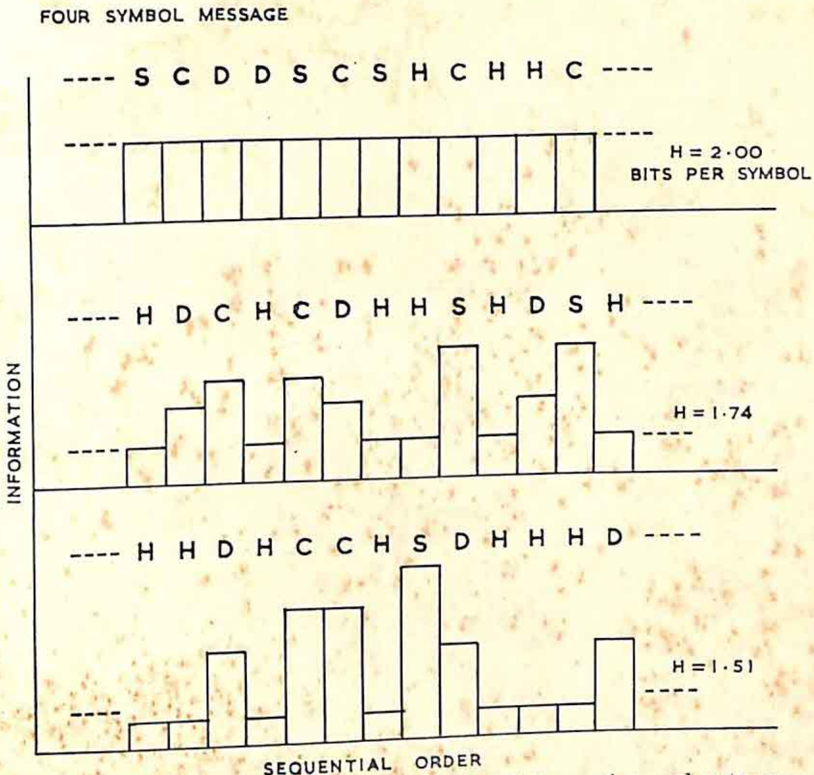

FIG. 1. Effect of Frequency unbalance on information and entropy

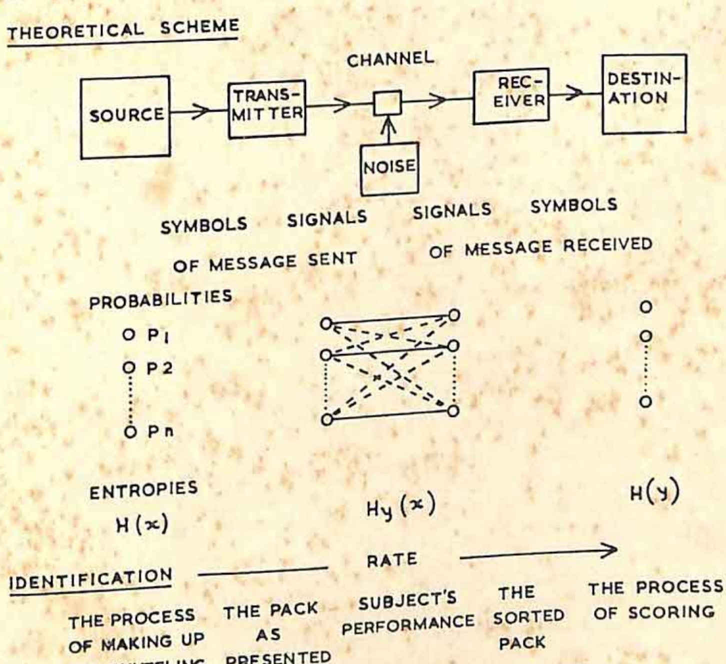

FIG. 2. The Theoretical Model and its Identification

If so, $R = (\log n - 0)$ bits per word, or $W \log n$ bits per second. But if he makes errors with entropy $H_y(x) = E$ bits per word (for calculation of $H_y(x)$ see Ref. (2)), $R = \log n - E$ bits per word, or $W (\log n - E)$ bits per second.

Any task in which a subject must respond to unpredictable variations of a display can be treated as above. There is a display-entropy $H(x)$, a response-entropy $H(y)$ and an entropy connecting display and response $H_y(x)$.

In calculations on choice-tasks it is convenient to analyse the total time-per-choice T_r as observed into two components, choice-time T_c and movement-time T_m, the latter being constant if all responses are made with closely similar movements. The choice-time depends on display entropy and it is this we are interested in here. We write—

$$T_r = T_m + T_c \quad \ldots \quad (4)$$

If T_c is a linear function of $H(x)$ and we can write

$$T_r = T_m + K\,(H(x) - H_y(x))$$

the rate of transmission, R, is given by

$$R = \frac{1}{K} = \frac{H(x) - H_y(x)}{T_r - T_m} \quad \ldots \quad (5)$$

III

Method

A sorting task was chosen as being the most appropriate. The subject makes a sequence of responses to signals which appear on the objects he is sorting. Each signal-response pair is like a choice-reaction time situation, but, compared with the experimental form of the latter, there are certain important differences. In the sorting task:

(1) The task as seen by the subject includes a complete set of signals, not just a single one, and he knows all there is to know about the statistical structure of the set. This is important because information is a property only of signals in a long sequence, and not of isolated ones.

(2) All signals requiring the same responses need not have the same physical form —"abstraction" can be studied.

(3) The subject has no uncertainty of *when* signals will occur since the timing is under his own control. (The correction, introduced by Hick, of writing $(n + 1)$ for n to allow for the "no-signal" possibility thus becomes unnecessary.)

(4) It is easy to give the signals in random order, however many are required.

(5) Much time is saved in scoring, and large numbers of choices can easily be introduced; no apparatus is needed and the task is familiar to most subjects.

But in the reaction-time task—

(6) Times can be obtained for each response and not only averages.

(7) Movement-time can be reduced to vanishing point.

For the present purpose we want only average times, and movement-time turns out to be constant to the required accuracy, so the sorting task was preferred.

An ordinary playing-card pack was chosen, being simple and familiar to most subjects. Usually the complete pack of 52 cards was used, and the classes to be sorted were chosen from colour, suit, court-or-plain, numbers, etc.; but composite packs were sometimes made up from two or more ordinary ones in order to obtain special values of entropy-per-card. The latter is calculated by equation (2), putting for the p_i initial relative frequencies (Table I). Strictly speaking, a bias is introduced by doing this, since the pack is not infinitely large, but the effect is small except in special cases and no mathematical correction has been made, but see (*a*) below.

In a sequential task such as this, the subject adjusts his speed to the point where errors are just avoided. It is on the whole desirable that a very few errors should occur since it suggests that the subject is working at maximum speed.

The experimental procedure was straightforward. Subjects were handed the pack face-downwards (or face-upwards in the "preview" condition) and told to sort it in some given way as quickly as possible. Misplaced cards were to be put right if noticed before turning up the next card. Appropriate sample cards were then put out on a squared board. The performance was timed on a stop-watch. At the end the sorted piles were checked for errors and reshuffled for the next trial. Some precautions were found necessary:

(*a*) When extremely unbalanced packs were in use, e.g., 4 aces from the rest, subjects tended to keep count of the smaller pile and when all were out to speed up considerably. This can be avoided in several ways. The chosen method was to insert one card of the small class among the last two or three cards after shuffling.

(*b*) Thorough shuffling was found to be important.

In order to obtain an estimate of movement-time, subjects were given a pack pre-arranged in a definite sequence, e.g., red-black-red-black, and told that it was so, and instructed to sort them just as in other tasks but using the information given. According to observation this was usually, but not always, successful in persuading subjects to make the same sequence of movements as in the real sorting tasks but without pauses for making decisions.

TABLE I

Entropy per card	*Number of classes*	*Task*				
0·00	(any)	Pack in pre-arranged order				
1·00	2	Red/Black				
1·55	3	Pictures/Red plain/Black plain				
2·00	4	Suits				
2·55	6	Red pictures/Black pictures. Rest in suits				
3·00	8	6 and below in suits, 7 and above in suits				
3·70	13	Numbers				
4·70	26	Numbers, each Red or Black				
0·78	2	Picture/plain				
0·51	2	Red picture/Rest				
0·39	2	Aces/Rest				
1·76	4	Red/Black picture, Red/Black plain				
1·47	4	Aces/Twos/Pictures/Rest				
1·14	4	Kings/Queens/Knaves/plain cards				
		A	B	C	D	(any characteristic)
0·47	2	45	5			
0·72	2	40	10			
1·00	2	25	25			
0·79	3	42	5	3		
1·15	3	35	10	5		
1·58	3	17	16	16		
1·57	4	30	10	5	5	
1·75	4	25	12	8	5	
2·00	4	13	13	12	12	

IV

RESULTS

Experiment 1. *Balanced Frequencies, Variable Number of Classes*

This experiment repeats the observations of Merkel and Hick in a different form of task, and it was not considered necessary to obtain highly accurate results. Figure III shows the results for one average subject out of the ten who have so far been tested. Each point plotted shows the time for the 2nd trial on that condition. The curve for the preview tasks provides some confirmation of the hypothesis expressed in equation (4) that one can analyse the total time into movement-time and choice-time. When the cards are sorted face-upwards, as soon as the subject has started to place one card the next comes into view and the process of deciding about it can start. Choice-time and movement-time can thus overlap as they cannot in the face-downwards condition. We expect from this analysis:

Face-down $T_{r1} = T_{m1} + T_{c1}$ Face-up $T_{r2} = T_{m2}$ if $T_{c2} < T_{m2}$

$= T_{c2} = K \log n$ if $T_{c2} > T_{m2}$

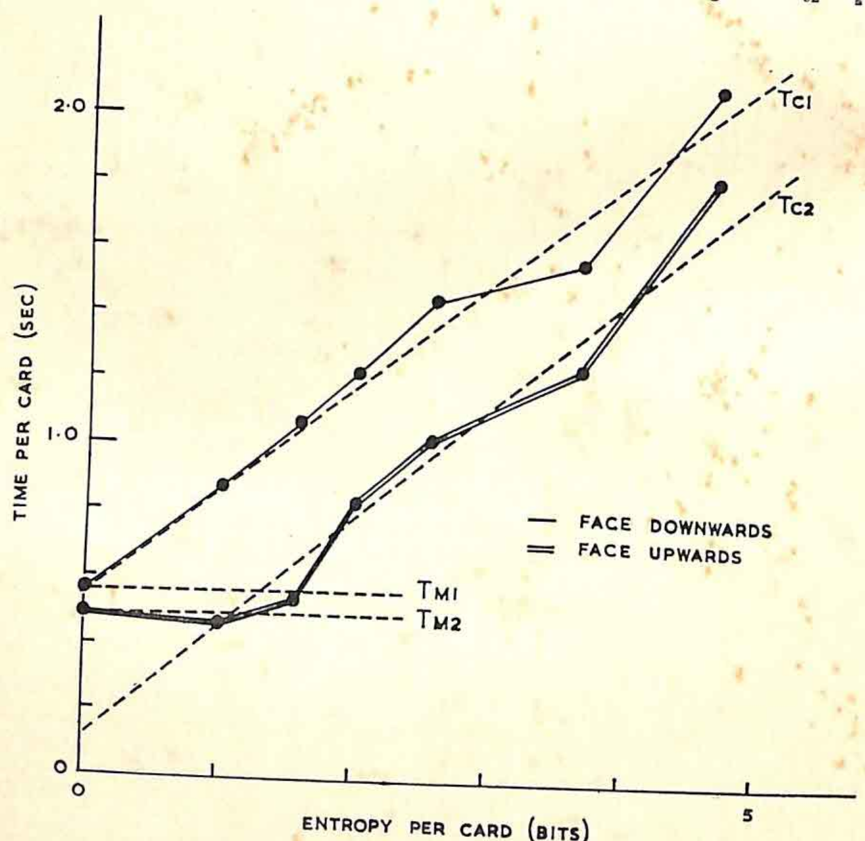

FIG. 3. Card-Sorting Balanced Frequencies. Expt. 1

This expectation is confirmed. We see in Figure 3 that a change of slope occurs at about $\log_2 n = 1{\cdot}25$; (equivalent number of classes $= 2{\cdot}5$) at which point $T_{m2} = T_{c2}$. Several incidental observations were made in the course of this experiment and will be mentioned later.

Experiment 2. *Unbalanced Frequencies,* 2 *and* 4 *Classes:* (*a*) *Statistical*

From the results of experiment 1, together with some other observations, a set of six tasks, three with two classes and three with four involving unbalanced frequencies was selected out of those possible using an unaltered pack (see Table I). With the corresponding balanced tasks and the movement-time measurements, there were nine conditions in all. Each of these was given five times, two practice and three experimental. The subjects left no errors uncorrected but the number of corrections was noted and, when these formed more than 25% of any class, the trial was discarded. Ten subjects were tested, the unbalanced conditions being presented in random order. The results are plotted in Figure 4 as a simple mean of all subjects.

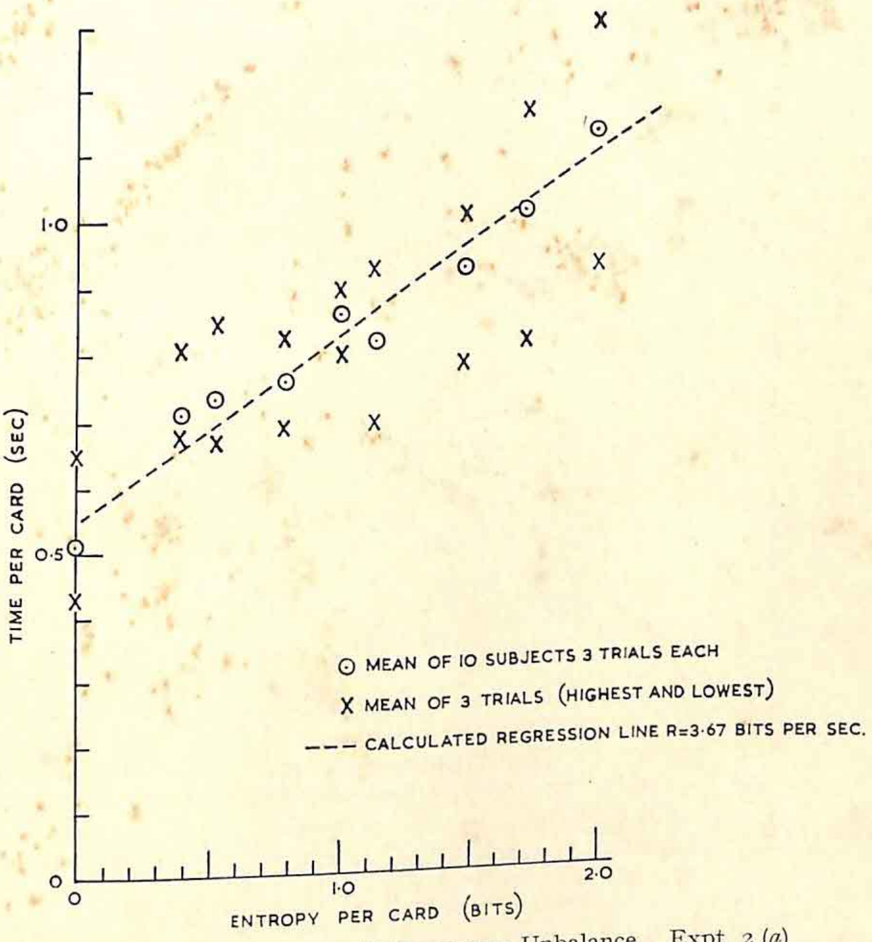

FIG. 4. Card-Sorting with Frequency Unbalance. Expt. 2 (*a*)

The relation between choice-time and entropy is clearly close ($r = 0{\cdot}86$, but the distribution of one variable was predetermined and not Gaussian). Analysis of variance, however, shows that the points depart significantly from the calculated mean regression line. No simple curve seems likely to fit much better than a straight line.

(*b*) *Intensive*

The previous observations were repeated on one subject, but on each condition much more practice was given so that a steady state was reached (15 or more packs on each condition). The conditions were given in random order on successive days. The results are plotted in Figure 5. The same pattern of results appeared as in experiment 2 (*a*). Practice effect, transfer between conditions, and individual differences could now be eliminated as causes of the observed departure from linearity. The remaining possibilities were either a real difference other than entropy between the 2-class and 4-class condition (e.g., in the ratio Q, equation (3)), or effects of differing difficulty of discrimination between the classes in different tasks. The former did not seem likely to account for all the discrepancy, but the latter might. In order to test the second of these, two composite packs were made up with equal frequencies of each of the pairs—aces/non-aces; pictures/plain, and times obtained. They differed significantly from that for red/black, though having the same entropy.

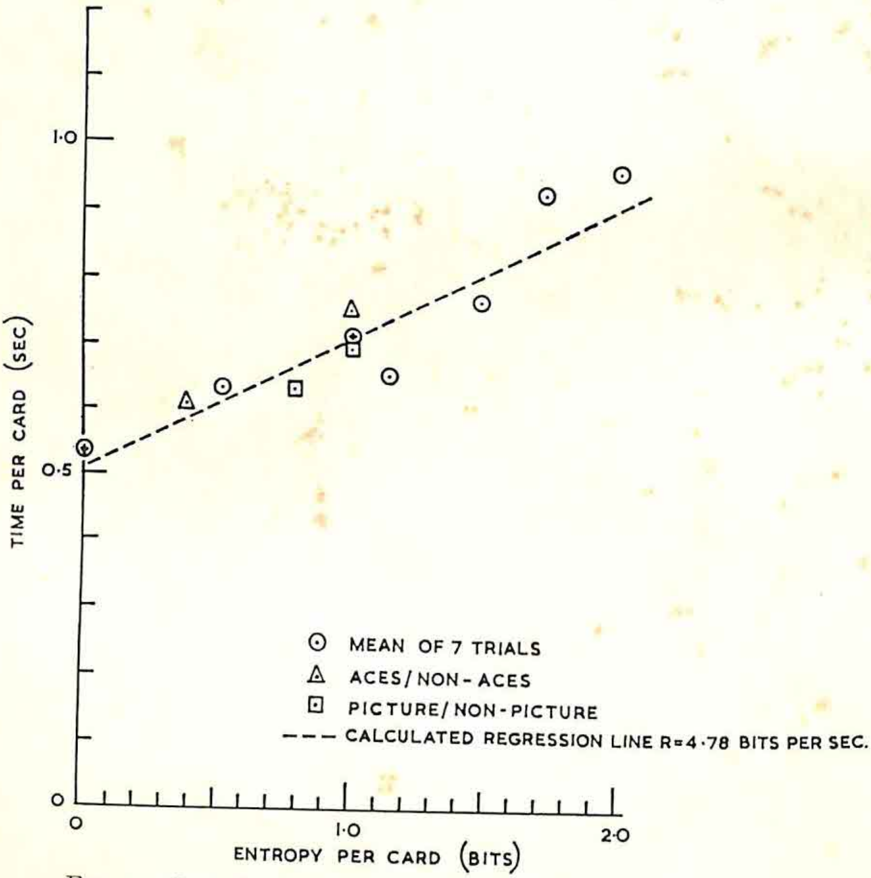

FIG. 5. Card-Sorting with Frequency Unbalance. Expt. 2 (*b*)

It was found that lines joining the new points to the corresponding ones for unbalanced conditions fell parallel to each other and to the mean regression line. This suggested that the difference in visual difficulty in discriminating between cards was giving rise to a difference in time-per-card independent of entropy. In principle, at least, all the departures from linearity might be accounted for in this way.

This finding shows up a major difficulty in the use of information theory in psychology, for information theory in the discrete case stated by Shannon says nothing about actual signals and the process of distinguishing them one from another; it deals only with abstract symbols already identified and distinct. Yet in all real information-carrying systems a signal must at some stage be examined and recognized as representing one of the expected symbols. This leads to the question, under what conditions of relative "discriminability" of signals might the theoretical structure outlined above be expected to hold? For one can obtain a wide range of choice-times at constant display entropy by making the task perceptually more or less difficult, the signals more or less discriminable. One cannot in general require "constant discriminability," since this depends partly on the set of other signals from which a given signal is to be distinguished and thus on n. This problem is avoided if one deals with the effect of frequency-unbalance on choice-time for one set of signals only. When this is so, rate can be properly defined. We may expect to find differences in rates so determined for different total numbers and different sets of signals.

Experiment 3: Unbalanced Frequencies: Constant Discrimination Difficulty

As a tentative solution to the problem and refinement of experiment 2, another set of tasks was worked out using only suits. Trials on all pairs of suits compared in balanced two-choice sorting showed only small differences in difficulty. Composite packs were made up using 2, 3, 4 suits, each in 3 degrees of unbalance, or 9 conditions in all (see Table I). These were given to two subjects. The results are shown in Figure 6.

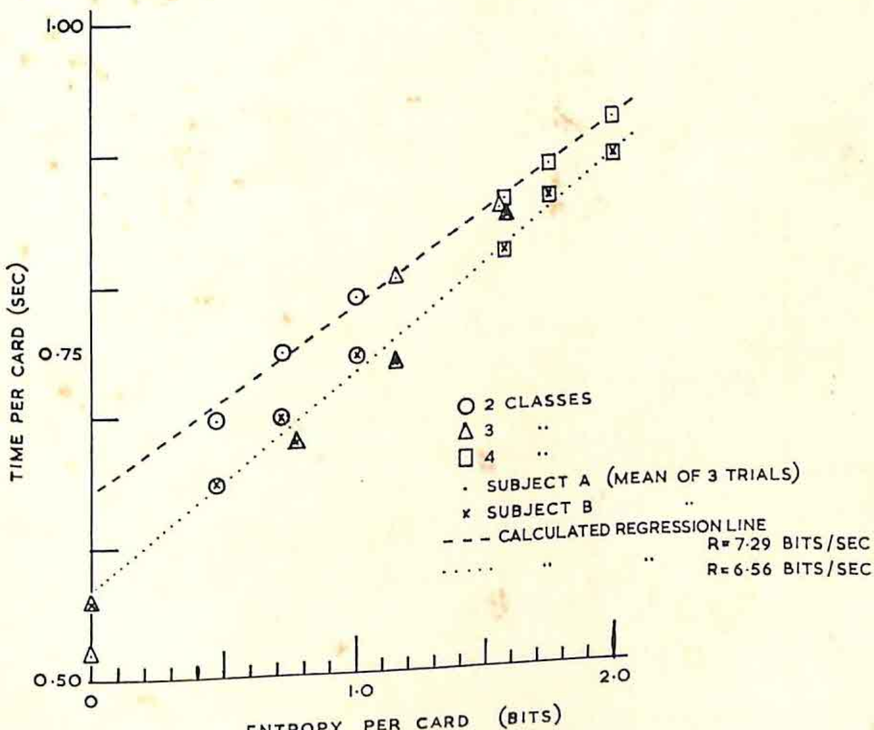

FIG. 6. Card-Sorting with Frequency Unbalance Constant "Discriminability." Expt. 3

Considering $n = 2, 3, 4$, separately for each subject, it will be seen that parallel straight lines fit all the points quite well. The line for $n = 3$, subject B, falls below the others, probably because in a three-choice task using suits, one class is of different colour from the other two, which makes discrimination somewhat easier.

V

Conclusions

One can consider that our hypothesis that rate is constant under variation of relative probabilities is upheld by these observations, with the proviso that "discriminability" of signals should be equal in a sense yet to be precisely defined. No systematic variation remains unaccounted for.

VI

Observations

Several incidental observations were made during the course of these experiments:

1. Learning curves for all tasks were of exponential form but with different parameters. Tasks with frequency-unbalance were very soon learned (one or two trials). Tasks in which sorting required two or more perceptual "degrees of freedom" to be combined—for example, red/black/pictures for the three-class task took longer to learn, but settled down to the predicted time after learning.

2. A task in which two perceptual degrees of freedom were crossed, i.e., red pictures with black plain/black pictures with red plain took a time corresponding to four classes, not to two, even after long practice. This may be regarded as evidence that the important factor governing choice-time is number of perceptual choices, not number of alternative responses.

3. The effect of bad shuffling was very marked. This would correspond theoretically to a reduction in entropy due to unequal transition probabilities between one card and the next (see Shannon), and one should be able to tackle it quantitatively.

4. Effect of position of heaps is small, being very quickly learned.

VII

Discussion

Some general concluding remarks may be in place. The most direct and clear-cut application which the results of studies based on information theory promise is a general quantitative measure of the difficulty and similarity of perceptual displays independent of their particular physical form. When this is achieved, it will remove the most important obstacle in the way of transfer-theory by making possible objective comparison of two tasks and so providing a truly independent variable with which transfer can be correlated. Before such general application is possible, however, several clear-cut problems must receive answers:

1. How precisely do differences in discriminability of signals affect their information content? And along with this. . . .

2. What effect does uncertainty of what precise form an expected signal will take have on its information content?

3. To what order of statistical structure (e.g., letter, word, sentence) can one extend the results?

4. How does information in various perceptual degrees of freedom (e.g., colour, shape, etc.) combine?

5. Under what conditions will the theory apply to continuous responses such as tracking?

Results bearing on questions 1, 3 and 5 have been obtained, which so far agree quite well with those expected from the theory.

The work was carried out under the general supervision of Dr. W. E. Hick, to whom the author's thanks are due.

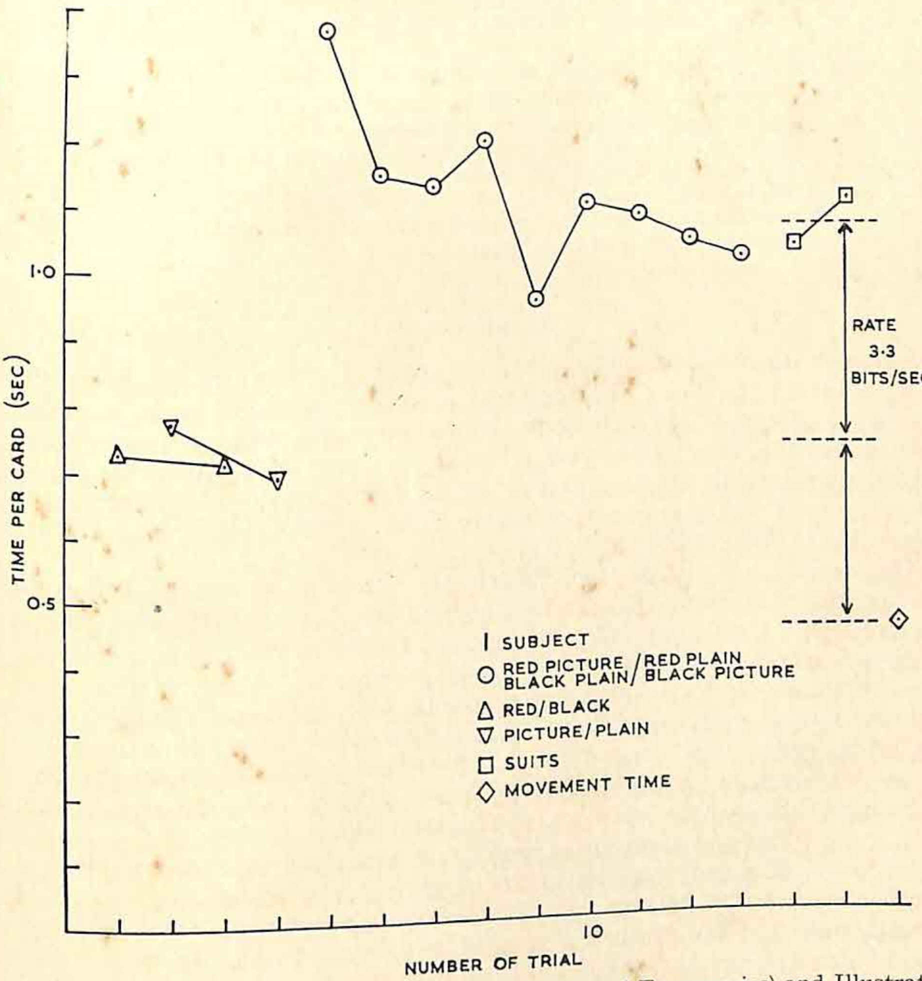

FIG. 7. Card-Sorting. Typical Learning Curve (Balanced Frequencies) and Illustration of Effect of "Crossed" Classification

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VISUAL FACTORS IN AUDITORY LOCALIZATION

BY

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Previous experiments showing the importance of visual factors in auditory localization are shown to have been insufficiently quantitative.

In the first Experiment, bells were rung and lights shone on the same or different vectors, eleven subjects indicating which bell had rung. In the second Experiment, a puff of steam was seen to issue from a kettle whistle with no whistling sound, while similar whistles were sounded by compressed air on that or another vector. Twenty-one subjects cooperated.

The addition of a visual stimulus at 0° deviation increased the percentage of correct responses significantly in the second, and insignificantly in the first experiment. At 20°–30° deviation the proportion of naive responses to the visual cue was 43 per cent. in the first and 97 per cent. in the second experiment. At greater angular deviations, the proportion of naive responses fell to chance level in the first, but remained significant in the second experiment, even at 90°. The "visuo-auditory threshold" was found to be 20°–30°, but might be much larger if there were more grounds for supposing the two stimuli to be from the same source in space.

I

Introduction

Although many experiments have been designed to elucidate the factors which enable a listener to locate the source of a sound, few have sought to assess the importance of visual cues. Of these the majority were "reversal" experiments, in which either the visual cues were reversed by means of prismatic spectacles (so that an object to the right appeared to the left) (Stratton, 1897; Ewart, 1930; Peterson, 1938), or the auditory cues were led to opposite ears by means of stethoscope tubes (Young, 1928, 1931; Willey, 1937).

The experiments of Stratton, Ewart and Peterson, in which the lenses were worn continuously for a number of days and which rotated the field 180°, were concerned with the learning of new responses to the distorted cues. Stratton and Peterson were each the only subject, but Ewart used three subjects. They all found that auditory localization was in accordance with current visual rather than auditory data—sounds were heard from the source as seen. Ewart, who distinguished "pure auditory" from "visuo-auditory" localization, stated that even after fourteen days' continuous visual reversal, "pure auditory" localization was unaffected.

Young (1928), whose "pseudophone" continuously reversed the auditory field 180°, found that normal vision compelled a return to normal auditory localization of the source of sound, but that in the absence of vision sounds were localized as then heard, that is, 180° reversed. In his second experiment, Young's (1931) five subjects, who did not wear the pseudophone continuously, sometimes reported "dissociation between the visible source and the 'phantom' sound" (p. 98).

Willey (1937) pointed out that, since the pseudophone allowed 10db direct stimulation by leakage at both ears, the resulting stimulation was by no means a simple reversal. He maintained that the auditory factors in visuo-auditory localization while wearing the "pseudophone" did not become reversed or reorganized, but merely suppressed, so that less notice was taken of them. Furthermore, visuo-auditory localization was entirely dependent on the associative powers of the subjects who showed "individual differences in susceptibility to visual suggestion."

* Working with a grant from the Medical Research Council.

Wallach's (1940) subjects correctly located sounds in the absence of vision. When a visual field of vertical stripes was rotated round them (cf Dodge, 1921) for a sufficient time, they perceived themselves to be moving, and the visual field to be stationary. This influenced their localization of sounds. Gemelli (1951) who has recently repeated this experiment with similar results gave a summary of experiments on the effects of vestibular stimulation on auditory localization.

Thomas (1941) found that if a light was flickering with the same frequency as the sound of a buzzer, subjects reported hearing the buzzer nearer to the light than was objectively the case. In his view, the identical rhythms of the light and buzzer constituted a "common fate," but the effect would occur only "under conditions in which the influenced modality is labile, and relatively weakly structured."

Witkin (1952) placed subjects in a sound-proof room. A window in this allowed them to see the experimenter reading aloud, the sound being led to their ears by tubes of alterable length. The subjects (97 in number) were asked to report when the sound no longer seemed to be coming from the reader's mouth. The results, which were expressed as differences in length of the two tubes, and converted to angles, were as follows:—

	Men	*Women*
Eyes Closed ..	5·2 cms. ≡ 17°	5·5 cms. ≡ 18°
Eyes Open ..	8·5 cms. ≡ 28°	11·3 cms. ≡ 38°

There was no significant difference between shifts from centre to the right, and from centre to the left.

This situation, in which sounds were led through tubes to earpieces in a sound-proof room, was highly artificial. Also the method, which was essentially that of "limits" suffered from the fact that the change was only from the centre, rather than either from the centre or from the periphery. It was not stated how the conversion to angles was achieved. In a similar experiment (not reported below), the writer found that, with no vision, some subjects did not notice the change in location of a bell moved continuously from 45° L to 45° R, using the apparatus and conditions of Experiment A (reported on p. 55).

The experiments just described have demonstrated the importance of visual factors in auditory localization. This prepotence is used in the practice of ventriloquism. Similarly systems of voice amplification in cathedrals and theatres employ the principle of "low-level sound distribution," in which a large number of loud-speakers distributed throughout the auditorium relay the actor's words, but at such low intensity that the exact source of sound cannot be located. In these circumstances, the visual cues that the actor is speaking suffice to make the listener hear the voice as if it came directly from him. Likewise, the voices of actors seen on cinema or television screens are heard to come from their mouths and to move with them, rather than from the stationary loudspeaker. This problem of "intimacy," as it is called by sound engineers, plays a large part in the design of cinema auditoria (Mason and Moir, 1941).

With the exception of Witkin's studies and an earlier investigation by Wooster (1923) who did not analyse her results statistically, there seems to have been no attempt to quantify the influence of vision on auditory localization, and to determine a "threshold of deviation" within which the two sets of cues are experienced as proceeding from the same source in space.

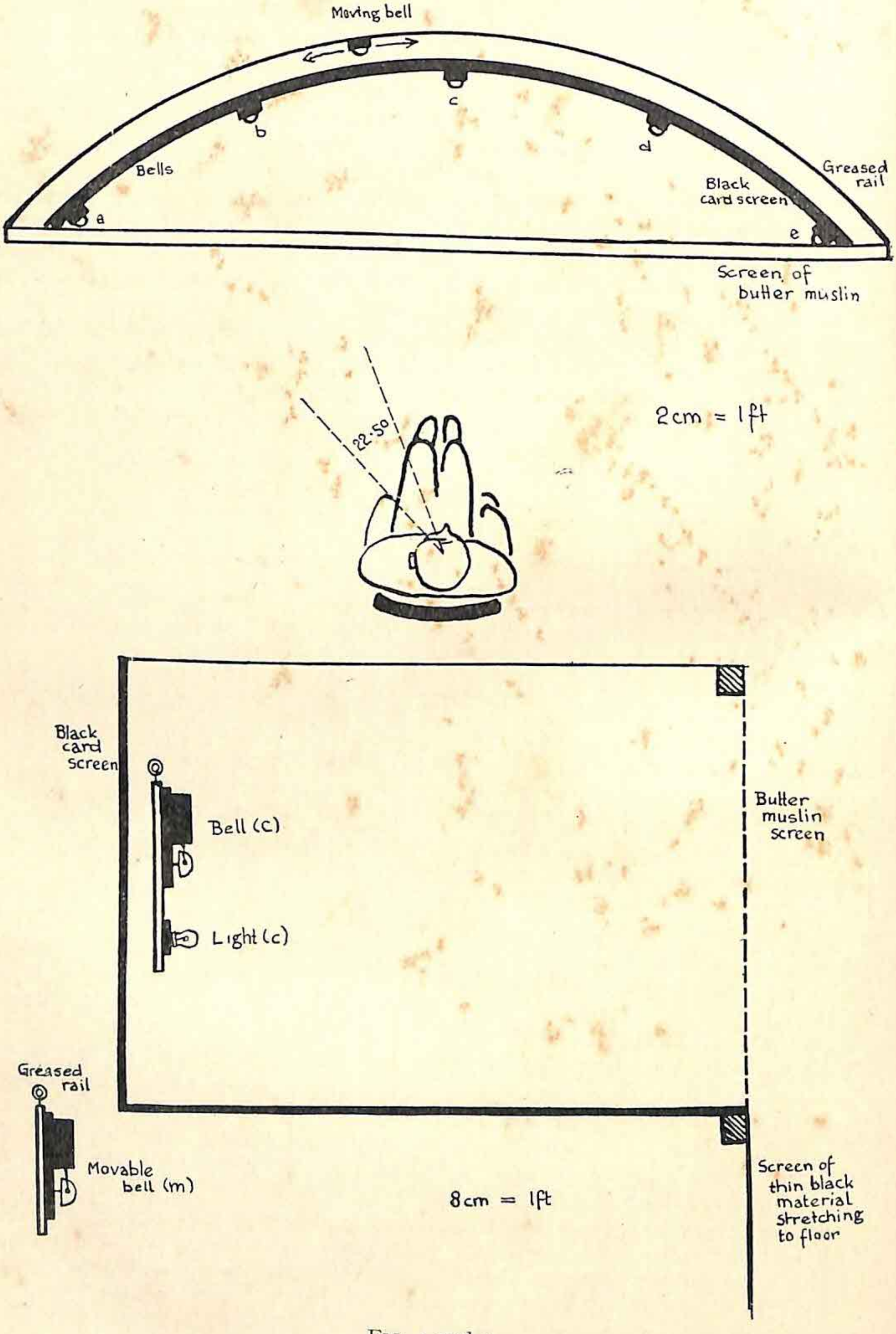
Moving bell
a
b
c
d
e
Bells
Black card screen
Greased rail
Screen of butter muslin
22·5°
2cm = 1ft
Black card screen
Bell (c)
Light (c)
Butter muslin screen
Greased rail
Movable bell (m)
8cm = 1ft
Screen of thin black material stretching to floor

FIG. 1 and 2.

The following experiment (A) was designed to elucidate these questions. The apparatus consisted of five bells and lights so arranged that every bell could be sounded when its light was shining, or independently of the light. However, it was realized that there were few reasons why the subject should suppose that his seeing a given light shining meant that that bell and not another was ringing. While physical and temporal contiguity might have given grounds for such a supposition to an ingenuous subject, a more sophisticated one would realize that the connections could be made at the will of the experimenter. Consequently the apparatus of Experiment B was set up, since it was considered that a subject, who saw a puff of steam rise from what he knew to be a steam kettle whistle, and who at the same time heard a whistling sound, had much stronger evidence for supposing that the two phenomena were connected.

II

Method—Experiment A

Apparatus

Five identical pieces ($1\frac{1}{2}$ in. × 4 in.) were cut from plywood. On each of these was mounted a miniature electric bell and a bakelite torchbulb-holder, all being of the same make and type. The five units were then hung on a semi-circular rail (radius 4ft. 3 in.), and obscured from the front by a screen of light butter muslin which permitted perfect vision of the bulbs when alight but only indistinct vision of the bells (Figs. 1 and 2).

Since preliminary trials revealed that the bells gave notes of widely different tone and intensity, a sixth such bell was mounted on a semi-circular rail (radius 4 ft. 6 in.) set up behind and below the 4 ft. 3 in. radius rail, in such a way that it could be moved along from one end to the other by means of a string and pulleys (Figs. 1 and 2).

A chair was placed so that the ears of the subject would be at the centre of the 4 ft. 3 in. and 4 ft. 6 in. radii, and the five bells and lights were connected with wires, so that they would appear to be capable of functioning. In fact, only the movable bell was wired up and the five lights, so that the movable bell M could be sounded on any of the five radii a-e on which the stationary bells were fixed, at intervals of $22\frac{1}{2}°$.

The circuit was so arranged, that the bell could be rung in the absence of a light, but would also ring whenever one of the five light switches was pressed. There was also provision for the continual sounding of a buzzer, except when the bell was being rung. The buzzer was placed on the floor in the central position c, and served to mask the sounds of adjusting the position of bell M.

Ten trials of each letter a-e were randomized. Three people were asked one after the other to stick a pin into three 10 × 5 squares, and the order was noted until the series reached completion. This order was used for the sounding of bell M only. A similar procedure was adopted in arranging a further random series for different locations of bell M, with one of the lights a-e shining.

Conduct of Experiment

The subject was brought into the experimental room, and was allowed to see the five bells a-e, together with their lights. He was seated in the chair, and this was adjusted until, when seated comfortably, his ears were 3 ft. 0 in. from the centre of the butter muslin screen (Fig. 1). He was blindfolded and then given the following instruction:—"As you have probably seen, there are five bells behind here, a-e. I am going to ring one at a time. Will you please say which bell is ringing. I will ring the bell for one second. Between the rings a buzzer will be sounding."

Bell M was then sounded ten times in each position a–e according to the random series (Series I). The blindfold was then removed and the following instructions given.

"In this part of the experiment, I am also going to shine a light. Sometimes this light will shine at the bell which is ringing, and sometimes at a different bell. Will you please say where the sound is coming from, that is which bell is ringing, and also which light is shining. As before, will you please tell me how sure you are of your *sound* judgment."

The bell M was then sounded ten times in each position a–e, so that for all positions of the bell the light was shone twice in each position a–e, and a complete table of 10 × 5 was filled. These trials constituted Series II.

Ten subjects took part in preliminary experiments with this apparatus (not reported) and eleven in the main experiment.

III

Results—Experiment A

Series I. The proportion of successes was submitted to analysis of variance, and marked position differences were revealed. Thus, the proportion of successes at position a averaged over all subjects was 86 per cent., but at b it was only 21 per cent., while that at the remaining three positions did not differ significantly from the overall mean value of 44 per cent., although c was better than d and e.

Of the incorrect judgments, 92 per cent. consisted of a response adjacent to the true position (22·5° error), but stimuli at positions c, d and e gave rise to significantly greater angular errors than those at a and b. A χ^2 test showed that this position effect was the same for all subjects. It may have been due to the orientation of the apparatus in the somewhat resonant room, since only bell M was rung throughout.

Series II. χ^2 tests applied to the proportion of successes of every subject separately in Series I and II showed that when a light shone at the same position as the sound (coincident stimuli), correct identification of the source of sound was not significantly improved (being 48 per cent. and 60 per cent., respectively). On the other hand, when the light shone at a different position from the bell (conflict stimuli), correct identification of the source of sound was worsened significantly ($P = 0{\cdot}02$), but the performance of all subjects showed marked individual differences, that of two of the eleven subjects actually improving in these circumstances.

Analysis of variance of the proportion of successful localizations of the source of sound in the conflict trials showed significant position differences similar to those of Series I ($P = 0{\cdot}05$), and significantly fewer correct localizations when the light was to the left of the sound than when to the right ($P = 0{\cdot}05$), but the actual angle of deviation (22·5°, 45°, etc.), was without effect. The fact that there were fewer correct localizations when the light was to the left of the sound was not related to the overall greater success with which sounds from position a (which had no light to its left) were located (Series I).

A regression of the position effects of Series II on the position effects of Series I was found to be significant ($P = 0{\cdot}05$), but in Series II there were significantly more successes at position c than in Series I and fewer at positions d and e.

Further χ^2 tests showed that errors of over 22·5° in locating the sound were significantly greater in Series II than in Series I.

During the conflict trials of Series II, reactions to the light stimulus occurred significantly more often when the objective deviation between light and sound was 22·5°, than when the deviation was 45° or more. Indeed, when more than 22·5° the angle of deviation did not significantly affect the proportion of responses to the light stimulus.

Thus the total proportion (all subjects and all trials) of incorrect results was not affected by the angle of deviation, but a much larger proportion of them were reactions to the light rather than to the sound when these were separated by only 22·5°.

It would appear that when the two stimuli were separated by only 22·5°, there was a strong tendency to associate them, but that when 45° or more apart there was no such tendency, and the sound was judged to have come from the direction of the light stimulus only as often as would be expected by chance.

Excluding those trials in which the response was a naive reaction to the light stimulus, and allowing for the fact that the opportunities to misplace the sound towards the light were more frequent than those to misplace it away from the light, there remained a significant tendency to misplace the sound towards the light (χ^2 significant at $P = 0{\cdot}05$).

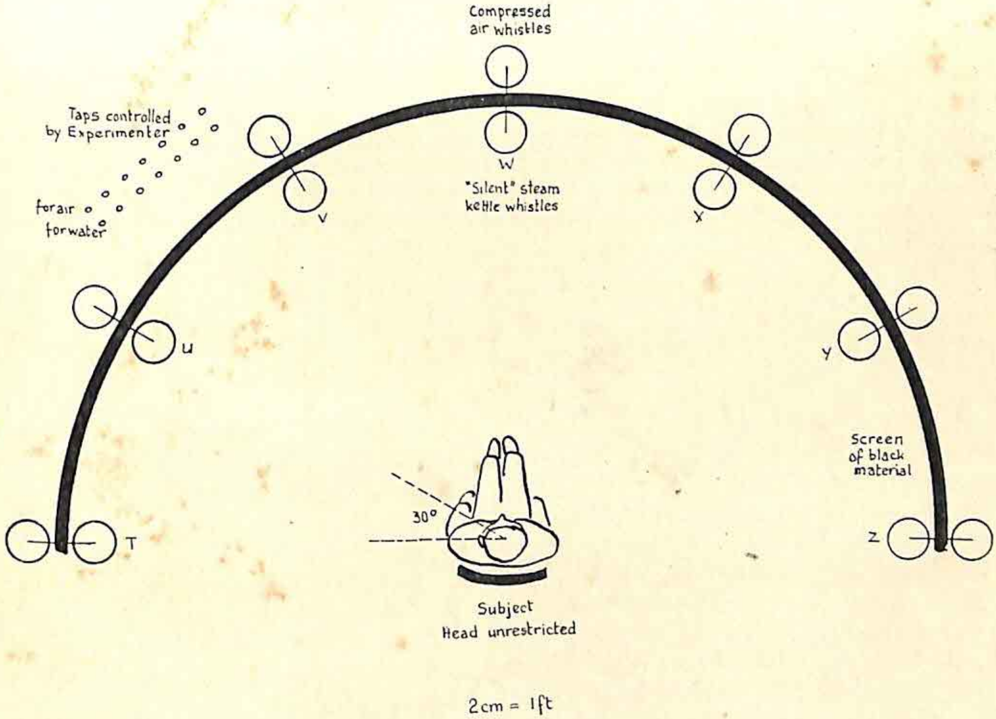

FIG. 3.

IV

METHOD—EXPERIMENT B

Apparatus

A chair was placed in a marked position on the floor, and round it seven aluminium kettle whistles were set up in a semi-circle of radius 4 ft. 6 in. at 30° intervals. The whistles were placed approximately at the ear level of subjects seated in the chair (3 ft. 8 in. from the floor) and their reeds destroyed (Fig. 3).

Three brass tubes each containing a 50-watt soldering iron element were inserted into the rear of each whistle. Water was led from a header tank through a battery of taps, controllable by the experimenter, to a fine jet which was pressed into a tuf-nol sleeve round the central of the three heater units in each whistle. Thus, when the heater units had been allowed to attain their working temperature, momentary release of one tap caused a visible cloud of steam to rise from the corresponding whistle. Unfortunately but unavoidably, the release of the steam (which was to serve as a visual cue only) was accompanied by slight sounds of hiss and spatter, but no true whistling sound came from these "silent" whistles.

Seven more whistles of the same type were set up level with those already described and on the same vectors, but 3 in. behind them—that is at radius 4 ft. 9 in. from the subject (Fig. 3). These were connected to a second battery of taps controllable by the experimenter and were supplied with compressed air through a reduction valve which ensured a supply of air at the optimum working pressure of the whistles (5–7 lbs./in.^{2}). Momentary release of one of these taps gave rise to a whistling sound from the corresponding whistle and closed an electric circuit thus starting a Venner Stop-Watch. This second set of whistles—the sounding ones—was hidden from the subject. It was therefore possible to emit a whistling sound from any of the seven vectors and at the same time to release a cloud of steam from a "silent whistle" on the same or a different vector.

The circuit to the stop-watch was broken when the subject responded and the watch stopped by means of a microphone placed on his throat, and an electronic sound key.

Compilation of Random Series

Fisher's tables of random numbers were used to prepare a series of four trials at each of the positions t–z (Series I). After some preliminary trials, it was decided that the series which included visual cues, should have two coincident stimuli to every conflict stimulus. Trials of stimuli were therefore prepared such that the order of each conflict stimulus was varied, and no two conflict stimuli occurred together. The sequence of both auditory and visual cues was randomized separately and then brought together. In Series II the angle between auditory and visual cue was 30°, in Series III, 60° and in Series IV, 90°.

V

Results—Experiment B

Correctness of Response

The results of the 21 subjects were first analysed as to correct or incorrect decisions. The majority of judgments in Series I were correct. This was to be expected, since the intervals between the whistles were 30° and earlier work such as that of Stevens (1937) had suggested that angles of 10° to 12° were discriminable auditorily in the open air at the frequency of these whistles (circ. 5–6 Kcs.).

There was, however, a marked centralising tendency of responses to stimuli at the extremities (positions t and z), but the next positions (u and y, v and x) showed the opposite—an exteriorizing tendency, which is unusual in such investigations. In conformity with the findings of previous workers, it was found that the central position (w) was localized best by these subjects.

That the responses to stimuli on the right hand side of the subject (whistles x, y and z) were slightly less accurate than those to stimuli on the left may have been due to the fact that the apparatus was placed more to the right of the room, and whistles x, y and z were in fact in the corner of the room, whereas whistles t, u and v were more central in the room. Undoubtedly the sounds of x, y and z were more subject to reflection from the nearby walls, which might well have masked some of the binaural differences by which presumably the subjects were judging the source of sound.

When the blindfold was removed and the visual and auditory cues were given on the same vector (coincident stimuli of Series II—that is two-thirds of the total number of stimuli in Series II), only three of the 457 (1 per cent.) responses were incorrect and these were removed only one whistle (30°) from the sounding one. Similarly only nine of 372 (2·5 per cent.) coincident responses in Series III, and 16 of 307 (5 per cent.) coincident responses in Series IV were incorrect. These incorrect responses were mostly given by one or two subjects who refused to heed the visual cues, and maintained a *purely* auditory localization throughout which was presumably subject to errors similar to those of Series I.

It was worthy of mention that once each in Series III and IV a single source of sound with visual cue on the same vector was reported as a double source. That is, the subject decided that two whistles had been blown simultaneously, although he had seen only one puff of steam. It may be that, because of the intervening conflictful stimuli, he had become set in attitude to expect some deviation between the location of the source of sound and its visual concomitant [cf. the effect of determined expectation reported by Geissler (1915)].

The responses to the conflictful stimuli of Series II, III and IV were analysed in detail statistically. Marked differences between subjects were found. When the responses of each individual subject were classified into correct and incorrect decisions, χ^2 with 20 degrees of freedom (21 subjects) had a value of 70·0 which was very highly significant. When the responses of individuals were classified as "vision accepting" or "vision rejecting" (regardless of whether the source of sound was correctly identified) χ^2 with 20 degrees of freedom was 81·0, even more highly significant.

The numbers of results from all subjects are shown below, converted to percentages.

Series	*Deviation between Auditory and Visual*	*Correct Auditory*	*Response to Visual*	*Correct Response to Neither*
II	30°	2·6	97·0	0·4
III	60°	28·1	61·9	10·0
IV	90°	50·2	36·7	13·1

It will be seen that the number of correct responses to the auditory cue and the number of totally incorrect responses rose when the angle of deviation between the source of sound and its visual concomitant was increased, while the number of naive responses to the visual cue showed a corresponding decrease.

Every conflict trial of Series II, III and IV for all subjects was classified according to the position of the auditory stimulus (t to z); the angle of deviation between the source of sound and its visual concomitant; and the direction of this deviation (to the right or to the left of the source of sound). The proportions of correct responses to the auditory cue by all subjects were then calculated under these headings, and transformed to angles by means of the Fisher and Yates formula:—

$$\theta = \sin^{-1}\sqrt{p}$$

An analysis of variance was carried out. It appeared that neither the position of the sound nor the direction of deviation had any effect and that the only factor which influenced the proportionate success (apart from the subject difference already noted) was the angle of deviation (F [2, 27] = 22·14, which is highly significant).

Comparison of the number of naive responses to the visual cue and the number of correct responses to the auditory cue gave a value of $\chi^2 = 171$ with 2 degrees of freedom which was highly significant, and corresponded with the marked decrease in frequency of naive response to the visual cue when the angle of deviation was increased. The value of χ^2 with 2 degrees of freedom on comparing the number of correct responses to the auditory stimulus and the number of totally incorrect responses was 1·43. This meant that once the deviation between the source of sound and its visual concomitant was detected, the amount of this deviation no longer affected the proportion of correct responses to the auditory cue.

Reaction Times

In the analysis of reaction times it was decided to ignore any possible effects due to the position of the auditory stimulus or to the direction of deviation, since these had been found to be of no importance in the analysis of proportionate successes in conflict trials of Series II, III and IV described above.

Every reaction time of all 21 subjects was tabulated into classes according to the angle of deviation between the source of sound and its visual concomitant, and whether it was a correct response to the auditory cue or an "expected incorrect" naive response to the visual cue, or was correct to neither. Within-class variances of reaction time were calculated, and found to differ significantly, with evident dependence on the class mean. All reaction times were therefore converted to logarithms, in which form within-class variances did not differ significantly from one another, so the analysis was completed using the transformed values.

On Bartlett's test of homogeneity all pairs of classifications were found to be non-orthogonal, so the analysis of variance was calculated by the method of fitting constants to represent the mean levels of the various factors. A sum of squares for subjects was removed first; this was significantly large when compared with the within-class variance. Afterwards, sums of squares for "angle of deviation, eliminating subjects, ignoring correctness of response to auditory cue" and for "correctness of response to auditory cue, eliminating subjects and angle of deviations" were removed, and found to be significant.

When, however, a sum of squares for "correctness of response to auditory cue, eliminating subjects and ignoring angles of deviation" was removed first and found to be highly significant, the remaining sum of squares corresponding to "angles of deviation, eliminating subjects and correctness of response to auditory cue" was not significant.

(All the sums of squares described as significant in the preceding paragraphs gave Values of F much larger than those required for significance at $P = 0{\cdot}001$.)

After removal of the sums of squares due to subject, correctness of response to auditory cue, and angle of deviation, there remained 73 degrees of freedom between classes. The sum of squares corresponding to these degrees of freedom was significantly large ($F\ [73, 465] = 2{\cdot}80$, significant at $P = 0{\cdot}001$), which suggested that some interaction between subjects and correctness of response was present. This might be expected in view of the different methods of response adopted by different subjects (reported below).

Further analysis indicated that there was no significant difference in reaction time between a correct response to the auditory cue and a completely incorrect response.

Thus, in summary, when the response was to the visual cue (which happened nearly always when the deviation was 30°, and with diminishing frequency when the deviation increased to 60° and 90°), the reaction time was short, and varied from subject to subject, but was not dependent on the angle of deviation. It might be inferred that in these instances the reaction was solely to the visual cue and involved no indecision or awareness of conflict.

When the subject suspected that the easily perceived visual cue did not necessarily conform with the source of sound, the reaction time was considerably longer, again varying from subject to subject, but not dependent on the angle of deviation. Nor did it depend on whether or not the answer given was a correct response to the auditory cue.

No tables of mean reaction times are shown, since means over all subjects would be misleading owing to the non-orthogonality, and a table of means for individual subjects shown separately would be extremely cumbersome.

Imagery

The imagery of each subject was classified as "more visual" or "less visual" depending on the amounts of other types of imagery he reported spontaneously, when asked to imagine himself in various situations. There proved to be no significant differences between the groups neither in respect of proportionate success nor reaction times.

Introspections

Three subjects reported that at first they reacted only to the sight of the steam. There was "no point in the experiment if you can see the steam," and they were "much more certain than when blindfolded." One used only hearing in the early series, resorting to vision only if doubtful and later discarding vision altogether. But the majority (14) throughout all series listened first to the sound, localized it, and then checked visually.

Six realized that the purpose of the experiment was to ascertain the effect of vision on hearing and the same number correctly separated the pure air whistle from the hiss of the steam puff, but as many as nine were convinced that two whistles were actually whistling particularly in Series IV. These results may correspond to Banister's (1925) "second sound."

None had a clear idea of what combinations of stimuli occurred and how they were varied, but four spontaneously expressed confidence that there had been no presentations with a deviation of 30°.

One subject "had not the faintest idea how steam comes out with no whistling" and another concluded that the experimenter was "blowing into seven tubes and spitting."

When they first took off the blindfold after completing Series I, twelve subjects were surprised to see how near the whistles were and one reported slight nausea following this discovery.

Comments worthy of quotation were:—"I felt distinctly confused and harrassed, since I did not know whether to rely on vision or hearing;" "I did not hear two sounds, but it seemed as if the sound was coming from elsewhere (than the steam puff), though this was *impossible;*" "I began to think I was 'hearing' things;" "I am sure I am imagining some;" "I noticed a detached feeling—the black background became dim and vague."

The last remark is similar to those occurring in a previous experiment (Jackson and Zangwill, 1952), which were compared with the phenomena of the depersonalized state.

VI

Discussion

In both Experiments A and B there were marked position differences. This was probably due to the fact that the experimental room was irregular in shape and highly resonant with a long reverberation time. However, it provided typical everyday conditions, suitable for a "real-life" investigation of auditory localization,

as distinct from the artificially abstracted conditions of a sound cage. The effect of accidental factors resulting from the sound characteristics of the experimental room, and, in B, the effect of unavoidable differences in pitch and tone quality of the whistles was minimized by the statistical analysis.

Audiometric examination of subjects was not undertaken, since it was considered that a person with a unilateral hearing loss learns to localize in spite of his deficiency. On questioning, no subject reported any known hearing loss. Any such slight losses would have been taken out in the subjects x subjects interaction.

The results are presented in the Table, expressed as percentages to the nearest whole number.

	No vision	*Angle of Deviation between source of sound and visual concomitant*							
		0°	10°	22·5°	30°	45°	60°	67·5°	90°
A	48	60		38 (43)		38 (20)		30 (11)	
B	62	99			3 (97)		29 (62)		38 (37)

The figures refer to the percentages of correct responses to the auditory cue, and the figures in brackets to the percentages of naive responses to the visual cue. It must be remembered, however, that the results of Experiments A and B are not strictly comparable, since the angle between adjacent positions in A was 22·5° and in B 30°. This may account for the apparently greater accuracy of response to the auditory cue in B with no vision.

Thus the accuracy of purely auditory localization was found to be 20° to 30°, in a normal room with 32 subjects. This is probably in agreement with Steven's (1938) figure of 10° to 12° obtained with two subjects in the open air, with no reflections.

There was some evidence that sounds from the centre (w in B) were located more accurately than those from the sides, but this was less apparent in A. As has been found by other experimenters, sounds from the sides in B with no vision were heard more to the front—a centralizing tendency—but a reverse effect also occurred. This exteriorizing tendency may have been connected with the fact that many subjects believed the whistles to be further away than there were.

As has been mentioned, the effect of coincident visual cues was to increase the accuracy of response significantly in B, but insignificantly in A. A visual stimulus at one unit of deviation drew the response towards itself and thus lessened the accuracy of response to the auditory stimulus. This failure to differentiate the separate locations of stimuli at 30° deviation may have resulted from the fact that pure auditory localization was accurate only to within 20° to 30°.

Visual stimuli at two or more units of deviation from the source of the sound caused little alteration of accuracy of response to the auditory stimulus under the conditions of A, but allowed some improvement to take place in B. When the difference between the units of deviation is taken into account, it is evident that visual factors had much more effect on auditory localization under the conditions and following the instructions of B.

Thus the addition of vision increased accuracy from 48 per cent. to 60 per cent. in A (12 per cent. increase not significant) and from 62 per cent. to 99 per cent. in B (37 per cent. increase significant). Similarly accuracy was 38 per cent. with 22° to 45° deviation in A and only 3 per cent. with 30° deviation in B; and with 67° deviation in A the number of naive visual responses was 11 per cent. (chance level), while that at 60° deviation in B was 62 per cent. Even at 90° deviation nearly one-third of the subjects had entirely failed to detect the separation and others were undecided.

There seemed to be no cataclyamic cut-off point beyond which all deviations were detected. The subject's discovery of deviation depended partly on his attitude —the more critical suspected subterfuge and some professed to pay no attention to the visual cue throughout the experiment—and partly on the experimental conditions.

The predominant imagery of a subject did not seem to affect his method of responding nor his results.

The usual method of response, as revealed by questioning during and after the experiment, was to locate the sound by ear and then to look round and check by eye that the judgment was correct. Most stated that they experienced some indecision when the visual check was contrary to their auditory impression, and methods of resolving this situation showed great individual differences.

Possibly connected with this method of response was an interesting habit noticed by the experimenter and by several subjects. During the blindfold series there was what amounted almost to a compulsion to turn the useless eyes in the direction of the heard sound. Some of Szafran's (1951) subjects showed a similar tendency. It would seem to indicate that there is a strong inclination to use every sensory modality to obtain the most accurate information about the environment. There would not seem to be any connection between this observation and the contention that eye movements affect auditory localization.

In general, the reaction times measured in B fell into two classes—the quick response to the visual cue and the slower response, in which the visual stimulus either played no part or merely hindered a purely auditory judgment. When the response was solely to the visual cue, both its correctness and its reaction time were independent of the angle of deviation between the source of sound and its visual concomitant. On the other hand, when the response was not a simple reaction to the visual cue, its reaction time was independent of the angle of deviation and its correctness subject to the same errors as with no vision, though there was some tendency to be influenced by the visual cue so that the sound was misplaced towards it.

That many subjects experienced serious indecision in some trials was evident from greatly increased reaction times and from their comments. It may be presumed that some were actually in a conflict, since a desire to comply with the experimental instructions would motivate a response to the auditory cue. On the other hand, the sight of steam emitted from such a well-known everyday object as a kettle whistle was strongly suggestive that that whistle had sounded. Thus both the auditory and the visual stimuli might be said to have "positive valency," and the subject to be in "attraction versus attraction" conflict.

Further evidence of a mild degree of intermodal conflict was evinced by the comments of several subjects who maintained that they "must be imagining," or that they noticed a "detached feeling," or even nausea. In fact, it would appear that on occasions when the subject perceived an auditory stimulus and a simultaneous

visual cue as separate, although supposedly from one and the same object, his perception had an illusory quality. It may therefore be suggested tentatively that, only when information relating to different qualities of an object mediated by various sensory modalities can be fully reconciled and integrated, is the object perceived as real.

Nine subjects insisted that in some conflict trials they had distinctly heard two whistles; one whistling sound came from the correct source, and another whistling sound was definitely imputed to the whistle which steamed. One of these subjects even heard two spatially separate whistles in coincident trials. These instances are reminiscent of the "phantom sounds" mentioned by Ewart (1930) and by Young (1928, p. 41) and of the "second sound" described by Banister (1925). They are also an example of ventriloquism, in which the predominance of the visual spatial cues misplaces the labile auditory localization. These observations also suggested that there is a tendency to perceive an object with all its usual qualities, even if only some of these are active stimuli at that moment. They agreed with the findings of Werner and Zeitz (1927) whose visual object was perceived as "scratching the wall" and showed Klemm (1909) to have been mistaken in dismissing as erroneous the results of one subject who perceived a single object, which had visual qualities and was also a source of sound. Indeed, it would seem that the presentation as cues of but few qualities of an object may give a perception of that object with all its usually attendant qualities.

Although Experiments A and B were not strictly comparable, some interesting suggestions arise from such a comparison. In Experiment A there were few reasons why the subject should suppose that, when one of the lights shone, the bell on that vector was ringing. There is little in past experience to connect the ringing of a bell with the shining of a light. Indeed, all but the most ingenuous subject would realize that any bell could be made to ring when any light was shone. Only the simultaneity with which these phenomena occurred was conducive to supposing a relation between them, and the instructions admitted that not all stimuli would be coincident.

On the other hand, in Experiment B, past experience of such common-place articles as kettle whistles, coupled with the sight of steam rising from one such whistle, provided stronger grounds for supposing that a whistling sound heard at the same time was emitted by that particular whistle, unless there were definite and incontrovertible evidence to the contrary.

Thus the greater effect of visual factors on the accuracy of auditory localization, which was apparent in Experiment B, might well have been a result of the stronger reasons for supposing the two cues to be related. Binet was of the opinion that suggestibility was governed by "habitual expectations." In Experiment B, past experience of the two phenomena simultaneously might have formed "habitual expectations," which would lead the more suggestible subjects to assume that the heard whistling, and the seen steam proceeded from one and the same whistle. Alternatively, the fact that, in past experience, the sound of such whistles and the sight of the steam have been experienced at the same time and as proceeding from the same object might have led to the assumption of some causal relationship between the two phenomena. In truth the passage of the steam does cause the reeds to vibrate, and the whistle to sound.

Michotte (1946) maintained that the "impression of causality" in the visual study of moving objects was "immediately given in perception." Michotte, however, was less decided about situations such as have been described above, and

remarked "it is evident that in the liaison of impressions from the different modalities, factors of integration are aroused in a manner less automatic and less coercive."

Indeed, the different effects of vision under the conditions of our experiments A and B indicate that in this type of relation factors of past experience are of great importance. Moreover, the reactions of those subjects who experienced a sound stimulus which was not in fact given (phantom sounds) suggest that, if the causative factors—in this case the steam—are sufficiently compelling, the effect—in this case the heard whistling sound—will also be experienced, even if it is not objectively presented.

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ASYMMETRY OF PERCEPTION OF SIZE BETWEEN THE RIGHT AND LEFT HANDS IN NORMAL SUBJECTS

BY

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A method of measuring the perception of size in the hands was applied to 40 right-handed and 40 left-handed normal subjects.

It was found that objects of equal size held simultaneously in each hand tend to be judged to be unequal, and that, in the majority of subjects, the object held in the dominant hand is perceived to be the smaller.

These results are discussed in the light of previous work, and some reference is made to the examination of pathological cases.

Introduction

The investigation was directed towards finding answers to the following questions:—

(*a*) Are objects of equal size held simultaneously in each hand judged to be equal?

(*b*) If equal objects are judged to be unequal, is this asymmetry of perception of size related to handedness?

We have been able to find only two references in the literature which deal with the problem of the asymmetry of perception of size in normal subjects. The first of these is an article by Hall and Hartwell (1884) in which it is stated that when a subject attempts to move both arms over an equal distance the arm on the side of the dominant hand moves a greater distance than that of the other arm. That is to say, distance is underestimated by the arm on the side of the dominant hand. The second is a statement by Sherrington (1900) that Bloch found ". . . distance is underestimated by the left hand in right-handed people, but by the right hand in left-handed people." In Bloch's test the subject held pages of a book between the thumb and forefinger of each hand.

Apparatus.

The test objects were circular, flat-edged discs, made of aluminium 1/16 inch thick. Two discs were 25/16 inch in diameter, and the remainder increased in diameter by 1/16 inch. Thus the series of discs were 25/16, 25/16, 26/16, 27/16, and 28/16 inch in diameter (referred to from now on as discs, 25, 25, 26, 27, 28). Pieces of lead were inserted into the centre of the discs to make them all of equal weight.

Procedure.

The subject who had not recently performed any vigorous arm exercise, was seated comfortably at a table with his forearms resting on the table, his eyes closed and with the thumb and forefinger of both hands held apart ready to receive a disc between them. He was presented with both discs simultaneously and was only allowed to hold the rims in contact with the skin of the tips of his thumbs and forefingers, and was instructed to roll the discs gently, without pressure, between the fingers.

His judgments were recorded on a chart like that shown in Fig. 1. This shows also the judgments given by one of our subjects, L meaning large, E equal and S smaller. Box A is a chart recording the results obtained when the subject holds a disc size 25 between the thumb and forefinger of the *Right* hand and discs varying from 25 to 28 simultaneously between the thumb and forefinger of the *Left* hand. Box B records the results of comparing discs size 25 in the *Left* hand, and discs varying 25 to 28 in the *Right* hand. A series of random numbers from 1 to 32 was selected for each individual subject and the number of the square corresponding to the random number was then selected for the test. For example, with a series of random numbers 7, 19, 16 and 22, the subject would receive:—

A 25 disc in the Right hand and a 27 disc in the Left hand; a 25 disc in the Left hand and a 28 disc in the Right hand; a 25 disc in both hands; and a 25 disc in the Left hand and a 27 disc in the Right hand.

FIGURE 1

25 disc in R hand

Size of discs in L hand	R	L	R	L
28	S 1	L 2	S 3	L 4
27	S 5	L 6	E 7	L 8
26	S 9	E 10	S 11	L 12
25	S 13	E 14	E 15	L 16

Box A

25 disc in L hand

Size of discs in R hand	R	L	R	L
28	L 17	S 18	L 19	S 20
27	E 21	S 22	L 23	S 24
26	L 25	E 26	L 27	S 28
25	E 29	E 30	E 31	E 32

Box B

For the squares occurring in the first and third vertical columns (labelled R in Fig. 1) the subject was asked, "How does the size of the disc in the Right hand compare with that in the Left?", and for the squares occurring in the 2nd and 4th vertical columns, "How does the Left compare with the Right?" This was done to avoid his discovering any relation between the size of the discs and the form of question. He was instructed to answer the question, "How does the Right compare with the Left?" in the same way as the question was put to him, for example, "The Right is larger than the Left", so that we could be sure that he understood the question accurately. He had been told previously that one disc could be equal to, smaller or larger than, the other, and he was asked to give his first impression of the comparison.

The 32 tests were performed in four series of eight tests, with a short rest of about three minutes between each group of eight tests in order to avoid fatigue, and with an interval of five to ten seconds between each test. Before the series of tests the subject had three or four practise tests under the same conditions in order to familiarize him with the procedure.

Method of Recording and Assessing the Results.

The results were first recorded as in Fig. 1, then they were re-expressed so that they represented the size of the disc in the Left hand compared with the Right, as in Fig. 2. It

FIGURE 2

25 disc in R hand

Sizes of discs in L hand	L	L	L	L
28	L 1	L 2	L 3	L 4
27	L 5	L 6	E 7	L 8
26	L 9	E 10	L 11	L 12
25	L 13	E 14	E 15	L 16

Box A

25 disc in L hand

Sizes of discs in R hand	L	L	L	L
28	S 17	S 18	S 19	S 20
27	E 21	S 22	S 23	S 24
26	S 25	E 26	S 27	S 28
25	E 29	E 30	E 31	E 32

Box B

is clear that the horizontal line number 28 in Box A in Fig. 2 can now be written as: L = 4 E = 0 S = 0, and the horizontal line 26 in Box B as: L = 0 E = 1 S = 3. In this way the results in Boxes A and B can be transposed into the numerical terms given in Table I.

TABLE I

Relation of size of discs expressed as Left compared to Right	*Judgments*		
	Larger	*Equal*	*Smaller*
+ 3	4	0	0
+ 2	3	1	0
+ 1	3	1	0
0	2	2	0
0	0	4	0
− 1	0	1	3
− 2	0	1	3
− 3	0	0	4

Scoring.

We then scored these results. The reason for employing a scoring system is that a subject who says, for example, that a 28 disc is smaller than a 25 is making a bigger error of judgment than if he says that they are equal. Similarly, a subject who says that a 28 disc is smaller than a 25 is making a greater error of judgment than if he says that a 27 or 26 is smaller than a 25 disc. These facts must be recognised in a scoring system.* Table II shows how the results in our example were scored.

TABLE II

JUDGMENTS

	Larger		*Equal*		*Smaller*	
Size	*No. of Answers*	*Score*	*No. of Answers*	*Score*	*No. of Answers*	*Score*
+ 3	.. 4 ..	X 1 = **4**	0 ..	X 32 = 0	0 ..	X 64 = 0
+ 2	.. 3 ..	X 2 = **6**	1 ..	X 16 = 16	0 ..	X 32 = 0
+ 1	.. 3 ..	X 4 = **12**	1 ..	X 8 = 8	0 ..	X 16 = 0
0	.. 2 ..	X 8 = **16**	2 ..	X 0 = 0	0 ..	X 8 = 0
0	.. 0 ..	X 8 = **0**	4 ..	X 0 = **0**	0 ..	X 8 = 0
− 1	.. 0 ..	X 16 = **0**	1 ..	X 8 = **8**	3 ..	X 4 = 12
− 2	.. 0 ..	X 32 = **0**	1 ..	X 16 = **16**	3 ..	X 2 = 6
− 3	.. 0 ..	X 64 = **0**	0 ..	X 32 = **0**	4 ..	X 1 = 4

As it can be seen, the scores printed in heavy type—and to be referred to as X scores—represent all the judgments by the subject in which he thought a disc from size 25 to size 28 in the left hand were equal to or larger than a 25 disc in the right hand. Similarly the scores in ordinary type—to be referred to as Y scores represent all those judgments in

* Comment by G. A. Barnard, Professor of Statistics, Imperial College of Science and Technology:—

The scoring system is, of course, arbitrary, in that instead of scores 1, 2, 4 . . ., 64, etc., scores 1, 3, 9 . . ., 729, etc., might have been used, or any other system of scores. Provided the resultant distributions did not depart violently from normality, the significance tests would be unaffected. The question of the appropriateness of any one particular scoring system would become important only if some attempt were made to measure, on an absolute basis, the magnitude of the bias.

which he thought that a disc from size 25 to size 28 in the left hand were equal to or smaller than a 25 disc in the right. In the example the total of the X scores is 62, and of the Y scores 46. The *bias* in this subject is, therefore, 16.

A positive bias represents the score of a subject who has a tendency to judge the size of the discs in the left hand to be greater than those in the right; a negative bias that of a subject who has a converse tendency.

Correlation with Handedness.

Handedness was assessed on the basis of tests of writing, drawing, polishing a table, throwing a ball, the relative neatness and rapidity of simultaneous alternating movements of the forearms and hands, and the strength of the grips of the right and left hands as tested by a dynamometer. The performance of these tests was compared with the subject's own assessment of his handedness. The subjects were then classified into two groups, + 2 representing a strongly right-handed person, + 1 a moderately right-handed person, and − 1 and − 2, moderately and strongly left-handed persons, respectively. While it was sometimes difficult to decide the degree of handedness, in no case was there any doubt whether the subject was right- or left-handed.

Subjects.

Forty right-handed and 40 left-handed medical students and hospital staff were tested. Of the 40 right-handed individuals, six were females, and of the 40 left-handed individuals, seven were females.

Results

The results are set out in detail in Table IV and in Figs. 3, 4, 5 and 6. The mean bias score of the right-handed subjects was 59·1, *s.d.* = 84·9. For this mean, $t = 4{\cdot}40$, and P less than 0·001. The mean bias score of the left-handed subjects was − 31·7, *s.d.* = 85·5, and for this mean, $t = 2{\cdot}34$, and P less than 0·05. For the difference in means, $t = 4{\cdot}80$, and P less than 0·001.

TABLE IV

	Right-handed					*Left-handed*			
	Handedness	*X scores*	*Y scores*	*Bias*		*Handedness*	*X scores*	*Y scores*	*Bias*
1 ..	1	268	− 6	262	1 ..	− 2	18	− 208	− 190
2 ..	2	236	− 6	230	2 ..	− 2	20	− 196	− 176
3 ..	2	240	− 38	202	3 ..	− 1	37	− 210	− 173
4 ..	2	172	− 23	149	4 ..	− 2	38	− 196	− 158
5 ..	2	164	− 20	144	5 ..	− 2	19	− 176	− 157
6 ..	2	156	− 15	141	6 ..	− 1	9	− 156	− 147
7 ..	2	156	− 18	138	7 ..	− 2	31	− 172	− 141
8 ..	2	160	− 37	123	8 ..	− 1	74	− 188	− 114
9 ..	1	152	− 32	120	9 ..	− 1	22	− 124	− 102
10 ..	2	136	− 16	120	10 ..	− 1	30	− 116	− 86
11 ..	2	148	− 32	116	11 ..	− 2	28	− 106	− 78
12 ..	2	140	− 26	114	12 ..	− 1	32	− 104	− 72
13 ..	2	174	− 61	113	13 ..	− 1	20	− 88	− 68
14 ..	2	136	− 49	87	14 ..	− 2	44	− 100	− 56
15 ..	2	154	− 73	81	15 ..	− 2	52	− 104	− 52
16 ..	2	112	− 40	72	16 ..	− 2	22	− 72	− 50
17 ..	2	120	− 51	69	17 ..	− 2	69	− 110	− 41
18 ..	2	100	− 34	66	18 ..	− 2	40	− 72	− 32
19 ..	2	80	− 28	52	19 ..	− 2	46	− 76	− 30
20 ..	2	92	− 44	48	20 ..	− 2	48	− 76	− 28
21 ..	2	92	− 47	45	21 ..	− 2	28	− 52	− 24
22 ..	2	76	− 34	42	22 ..	− 2	44	− 64	− 20
23 ..	2	90	− 56	34	23 ..	− 1	52	− 68	− 16
24 ..	2	92	− 60	32	24 ..	− 2	72	− 82	− 10
25 ..	2	64	− 34	30	25 ..	− 2	44	− 48	− 4
26 ..	2	118	− 93	25	26 ..	− 2	62	− 60	2
27 ..	2	82	− 58	24	27 ..	− 2	82	− 78	4
28 ..	2	68	− 52	16	28 ..	− 2	64	− 56	8
29 ..	2	112	− 98	14	29 ..	− 1	68	− 50	8
30 ..	2	44	− 36	8	30 ..	− 1	64	− 52	12
31 ..	1	106	− 99	7	31 ..	− 2	62	− 46	16
32 ..	2	58	− 58	0	32 ..	− 1	80	− 48	32
33 ..	2	36	− 36	0	33 ..	− 2	76	− 32	44
34 ..	2	60	− 66	− 6	34 ..	− 2	94	− 48	46
35 ..	2	60	− 72	− 12	35 ..	− 1	94	− 42	52
36 ..	2	74	− 90	− 16	36 ..	− 2	118	− 59	59
37 ..	2	56	− 88	− 32	37 ..	− 2	92	− 24	68
38 ..	2	32	− 68	− 36	38 ..	− 2	165	− 85	80
39 ..	2	33	− 116	− 83	39 ..	− 2	152	− 16	136
40....	2	19	− 196	− 117	40 ..	− 2	216	− 27	189

Fig. 3 presents the bias scores graphically, each line representing one subject.

FIGURE 3

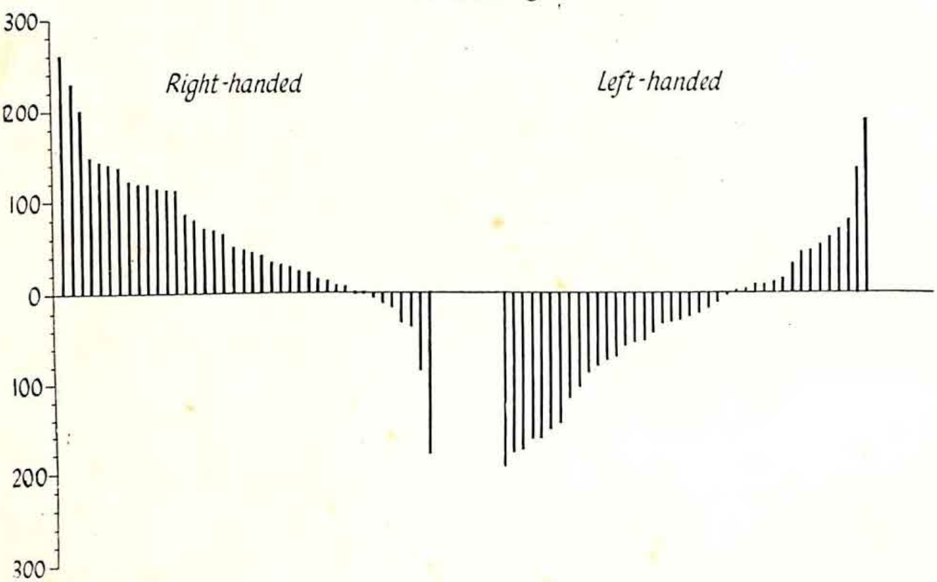
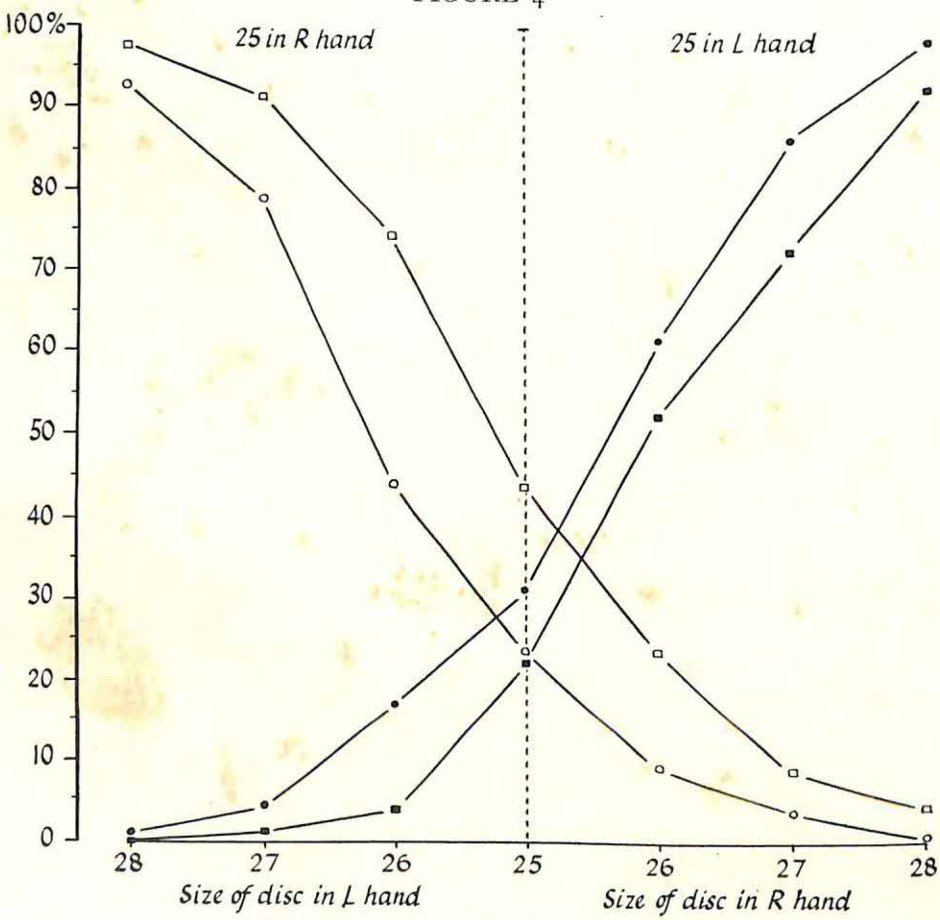


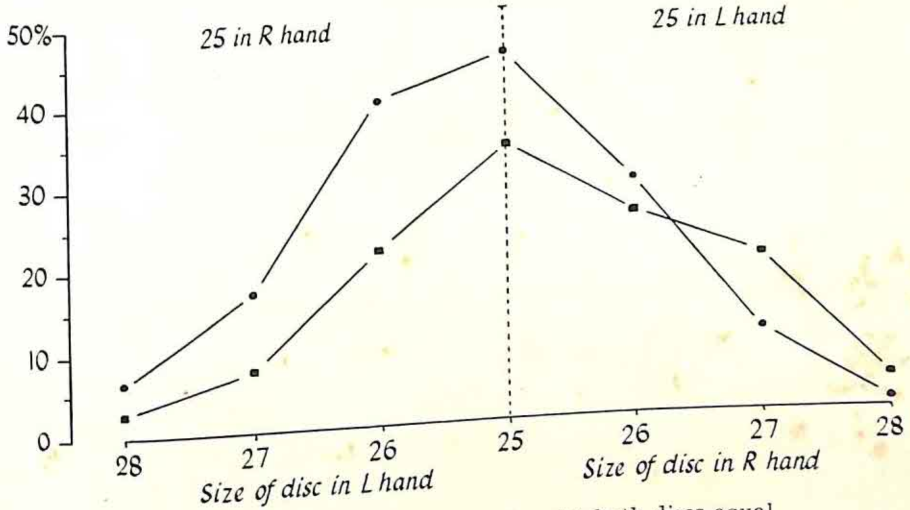
FIGURE 4



□ = R handed persons who thought L disc larger
○ = L handed persons who thought L disc larger
■ = R handed persons who thought L disc smaller
● = L handed persons who thought L disc smaller

These results may be expressed graphically in another form in which is expressed: (*a*) the percentage of the total number of tests in which all right- and left-handed subjects respectively, judged the Left disc to be smaller than the Right throughout the whole series of comparisons (Fig. 4); and (*b*) the percentage of the total number of tests in which all the right-handed and left-handed subjects respectively, judged the Left disc to be larger than the Right throughout the whole series of comparisons (Fig. 4); and (*c*) the percentage of the total number of tests in which all the right- and left-handed subjects respectively judged the Left disc to be equal to the Right throughout the whole series of comparisons (Fig. 5).

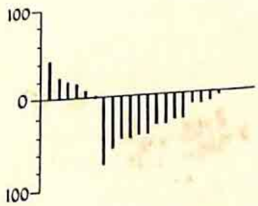
FIGURE 5



● = L handed persons who thought both discs equal
■ = R handed persons who thought both discs equal

In order to determine if any one individual gave consistent answers in the tests, 20 complete tests were performed on one subject. The results are shown in Fig. 6. This mean score was $-$ 17·1, *s.d.* = 30·1; t = 2·54 and *P* less than 0·02.

FIGURE 6



The bias score in 20 tests on 1 subject

Conclusions

Given two equal objects there is a tendency to judge them unequal. The majority of right-handed persons judge the right object to be smaller than the left, and the left object to be larger than the right. Conversely, the majority of left-handed persons judge the left object to be smaller than the right and the right object to be larger than the left.

Similar tendencies are shown when the objects are unequal. The tendency for the majority of right-handed persons to judge the right object to be smaller and the left object to be larger, is still manifested when the object in the right hand is in fact larger. Conversely, the tendency for the majority of left-handed subjects to judge the left object to be smaller and the right object to be larger is still manifested when the object in the left hand is in fact larger.

These tendencies vary from individual to individual within the right-handed and left-handed groups and cannot be correlated with the degree of handedness.

Any right-handed individual whose score falls within + 268 to − 196 must be considered to show a normal score, as must any left-handed individual whose score falls within − 210 to + 216. Any individual whose score falls outside the limits for his handedness group, can, at the most, be held to be only suspect of being abnormal.

Since right-handed persons tend to have a stronger right hand, and left-handed persons a stronger left-hand, it follows that, in all the above conclusions, strength may be substituted for handedness.

Since no correlation between handedness and eye dominance can be demonstrated, it follows that there is no correlation between asymmetry of perception of size and eye dominance. (Eye dominance was tested in the above experiments, but has not been reported upon.)

Discussion

The dissimilarity of function between the two hemispheres has received increasing attention since it was first shown that the function of speech is subserved largely, if not completely, by one hemisphere, and the literature which has grown up around the subject is considerable. A large part of this literature is devotèd to the unilateral localization of such functions as speech and spatial orientation. Another part is concerned chiefly with the comparison of the dexterity of movement of symmetrically opposite parts of the body, usually the hands. A third part deals with the comparison of sensory, as opposed to motor, function, between two symmetrically opposite parts of the body, usually the hands. It is this third problem which concerns us here.

In examining sensory function, this problem may be approached in several ways. Firstly, the threshold of the various basic modalities of sensation may be assessed with such apparatus as V. Frey's hairs, algesiometers and variable sources of light. Of this little need be said, for the punctate nature and pattern of distribution of peripheral sensory mechanisms makes the selection of symmetrically opposite equivalent points for stimulation impossible. This difficulty can, of course, be overcome by repeated testing over symmetrical areas, although, again, local conditions such as thickness of skin, or refractive errors in the case of vision, tend to invalidate the results. Moreover the technique of measurement of threshold values of sensation is difficult.

Secondly, the accuracy of discrimination between successive stimuli in terms of time, localization, and intensity may be compared on the two sides. This suffers from the same defect as threshold stimulation because the method is difficult to standardize.

Thirdly, the accuracy of discrimination between two simultaneous stimuli can be compared on the two sides. The technique of two-point discrimination is well established and needs no description here.

Fleischhacker (1947) has reviewed the various investigations in which the above methods of comparing sensory function were used. It is evident that the various investigators were at pains to compare the efficiency of sensory function on the two sides and their conclusions were generally phrased in terms of one hand being better than the other in some particular form of testing. Out of this, discussions of rivalry

between the hemispheres have arisen and the unsatisfactory conclusion has emerged that (as stated by Fleischhacker) ". . . possibly the superiority of the right or 'dominant' hand does not hold good for all sensory function."

This conception of efficiency of sensory function, of rivalry between and dominance of one or other of the cerebral hemispheres may have some justification. However, we did not find it helpful in framing the present investigation. We were not concerned with the efficiency or accuracy of one hand compared with the other, for the problem, as stated in the introduction, was "are objects of equal size in each hand judged to be equal?" It is obvious that, should one be judged smaller or larger than the other, the error is not one of perception but of judgment of two perceptions.

The comparison of our results with those of previous workers is not easy, for the methods employed are not strictly comparable. Bloch's method of testing perception of size by holding the pages of a book between the thumb and forefinger is indeed the only one in which a comparison is reasonable. We have not been able to trace his original paper, and Sherrington's statement that Bloch found " . . . distance is underestimated with the left hand in right-handed people, but by the right hand in left-handed people," is so much at variance with our results that the suspicion of a misquotation is raised in our minds.

In Hall and Hartwell's method, the subject, starting from a point midway in front of him, attempts, with his eyes shut, to move each arm simultaneously outwards over an equal distance. These investigators concluded, on the basis of the study of only four cases, that " . . . the preferred hand makes the greater excursion." If one can equate the perception of the distance moved by a limb and the perception of size of an object held in the hand then our results are in agreement.

Our results have enabled us to correlate the asymmetry of the perception of size with handedness and strength of the grip, but neither handedness nor strength explains the phenomenon. Indeed the exceptions in which the asymmetry of perception is reversed in relation to handedness and strength (that is, the object in dominant hand appears larger), invalidates the explanation that an object feels smaller because it is held in the dominant and stronger hand.

Studies in Patients with Brain Lesions.

We have made some studies of pathological cases in the hope that an abnormal asymmetry in the perception of size may prove of value as a clinical test of cerebral dysfunction. Although we are still pursuing this line of investigation, it is pertinent at this stage of the discussion to quote the results of some investigations of patients with parietal-lobe lesions. These results have demonstrated that many of such patients have scores outside the limits of normality which we have already defined. It is interesting to note that we have been unable to relate the side of the lesion to the direction of the asymmetry of perception. For example, a right-handed patient with a right-parietal lesion who had an abnormal weakness of the left hand, had a bias of + 311, whereas a right-handed patient with a left-parietal lesion who had abnormal weakness of the right hand, had a bias of + 288. Thus in these two right-handed patients, neither the side of the lesion, nor the strength of the hands, appeared to determine the direction of the bias. In other words, the direction of the bias gives no indication of the side of the lesion. We may also add that in patients studied there was no quantitative relationship between the degree of defect of postural and two-point perception and the degree of asymmetry of perception of size.

Halpern (1945, 1948) has published cases in which there was asymmetry of perception of size due to disease of the nervous system. He referred to the perception

of an article being larger or smaller than the one on the opposite side as macrostereognosis and microstereognosis repesctively, and he compared this abnormality of the perception of size of objects held in the hands with the visual phenomena of macropsia and micropsia. While his terms are accurate, we prefer "macrognosia" and "micrognosia" as more general terms which can be applied to the phenomenon of asymmetry of perception of size without reference to the types of sensation subserving the perception. Such terms are useful in descriptive work as long as it is not forgotten that their application in any particular case is purely arbitrary. Thus a case which is said to show macrognosia on one side may equally well be said to show micrognosia on the other.

Hall and Hartwell in their experiments on four subjects showed that, in a test in which the subject has to place two objects at equal distances on either side of a central point by means of visual judgment, the object on the side of the dominant hand is placed at a greater distance from the centre than the one on the opposite side. We have found that it is extremely difficult to judge the size of objects with any accuracy when macular fixation falls between them for the objects appear blurred in extra-foveal vision. However, we have examined a man with a left parietal glioma who spontaneously remarked, while having his visual fields charted, that a test object seemed smaller when placed on the right side of the screen. When examined with cards on which were drawn circles of size equal to the discs used in manual examination, it was found that the circles in the right field of vision were judged to be smaller than those on the left. With the manual test, he tended to judge the discs in the right hand to be smaller than those in the left, and this asymmetry was much in excess of that found in any normal subject. A comparison of the results of the visual and manual tests showed that the bias was roughly of the same order in each. From this and other similar studies we are led to speculate that, when the perception of a quality of an object can be subserved by more than one sensory mode (e.g. visual and manual), the asymmetry of perception between right and left is in the same direction and of the same degree in each case.

Acknowledgements

Our thanks are due to Professors Davis, Garry, Goldby, McNee, and J. Z. Young, who gave us facilities to test their students. We are most grateful to the students who were tested.

We should particularly like to thank Professor Barnard for his invaluable help and advice on the statistical aspects of the investigation, and also Dr. McDonald Critchley for his encouragement and criticism.

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THE PART PLAYED BY INTELLECTUAL PROCESSES IN A SENSORI-MOTOR PERFORMANCE

BY

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An experimental method is described for investigating the relation between intellectual processes and hand movements as both develop during the learning of a skilled task. Subjects were required to make predictive movements to join a series of circles; these appeared one at a time and were arranged in a pattern which was repeated thirty-two times. Comparison between the record of predictive movements and the subject's drawing of the pattern, made at the end of the experiment, showed that errors in the drawing corresponded to the type of response first made when the whole pattern was unfamiliar. For this reason it is suggested that the making of the responses played a part in the development of the idea of the pattern of circles, but the relation between the two is not a simple one.

I

Problem

Little is known about the change in, and development of, intellectual processes in the course of learning sensori-motor tasks. Tasks of this kind are often treated in isolation from the "higher" mental processes; and there is, perhaps, a reason for this. It is difficult to record directly changes in perception and concept while learning a skill.

Changes in perception and concept can be investigated however by an indirect method which depends on assuming that they are revealed in the character of the record of movements and its relation to the objective situation. This evidence can be checked by introspections and by other means. The method also depends on relating changes in the intellectual processes to the motor response, since the interaction of the motor and intellectual processes is of primary importance.

It is known that learning cannot take place without activity, but not known how the intellectual changes occur in relation to this activity. It is this problem which is to be considered.

II

Task and Procedure

A task was devised which requires a simple response, but one which to be successful has to become anticipatory, thus allowing for the building up of the intellectual element, and providing some opportunity for detecting and assessing the intellectual activity at various stages in the subject's performance.

It will have to be assumed that the nature of the motor response (e.g. whether it is anticipatory or not) provides some clue as to the stage of learning. It will be necessary to look for changes in the performance which indicate what stage has been reached. The existence of these stages, inferred from changes in the response, can be checked by other means.

The subject sat facing a screen, in which was a 6-in. vertical slit, 7·5 mm. wide. Behind the slit could be seen a single small black circle, 2·5 mm. in diameter, drawn on white paper. The subject was given the following instructions:—

"You look at this slit and you will see this one circle. When I switch on, the paper will move and the circle will cross the slit and disappear in this direction. The circle is the first of a series of circles, which will appear one at a time. They may appear in any position in the slit. You have to try to draw a pencil line through the circles. You hold the pencil point always against the right hand side of the slit, and you can move it up and down. Try not to follow the circles as they move across the slit, but to be ready for each one when it appears."

The paper band on which the circles were drawn moved from right to left. The circles therefore appeared without any warning from the side of the slit against which the subject was told to hold his pencil point. In order to hit a circle at all he had to predict its position, and move the pencil point to the part of the slit where it would appear. It was left to the subjects themselves to find out how the circles were arranged, and they were not told that there was a repeating pattern to be learnt.

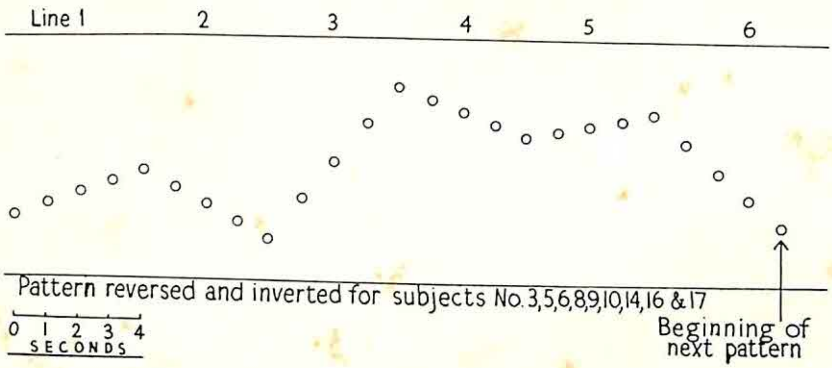


FIG. 1. Pattern of circles.

The circles were arranged on the paper band in a repeating pattern of twenty-four (Fig. 1). Spaced at equal time intervals of 1 second, they lay on six straight lines, so that, not counting the first circle, there were always four, and then a change of direction. The lines were all at different angles from the vertical, in an irregular zig-zag. The slope of the line determined the rate of the required response, so that some parts of the pattern were "fast," and some "slow." The pattern was repeated just over four times in each record. There were in all eight records, and in each one the pattern began in a different place. At the end of the experiment subjects were asked to draw their idea of the pattern, and after each record they were asked for their introspections.

The subject's performance was scored by two methods. The first was simply to count the number of circles touched at any point by the pencil mark. The second was to go through each record, circle by circle, and decide from the tracing what the subject had been doing while waiting for the appearance of the next circle; had he, for example, made a "correct prediction" (a movement towards the circle), or an "incorrect prediction" (a movement away from the circle), or a "waiting response," in fact, no movement at all, leaving him in the position of the previous circle.

There were twenty subjects, of whom eleven were psychology students, six were research workers, two were air mechanics of the Fleet Air Arm, and one was a housewife.

III

RESULTS

Scrutiny of the records revealed three stages in the learning of the skill.

First stage. At this stage the response was of the "waiting" type. This was made under two different conditions, first when the position of the next circle was not known, and secondly as a corrective movement when a mistake had been made. It was not a possible solution to the problem of hitting the circles, and had in fact to be abandoned before a successful prediction could be made.

Second stage. The response was now of the "predictive" type. Most subjects began to predict quite readily, and early in the experiment. After seeing one or two circles appear in line with one another, it was not difficult to predict another in the same line. This type of response led inevitably to overshooting at the points in the pattern where changes of direction occur, so that the pencil record often showed three correct predictions followed by an incorrect prediction (an overshoot), which was in line with the four preceding circles, although not with the whole pattern. This type marked an important stage in the learning because it emphasized the key points of the pattern—the corners which were continually overshot. It appeared that subjects who became conscious of these recurrent errors of overshooting, and who made some effort to avoid them, often managed to learn and predict correctly, usually by actually counting the circles, and turning the corner after the fourth. (A correct prediction at a corner will be referred to as a "correct anticipation," and a similar response, but made one circle too soon, as an "incorrect anticipation.")

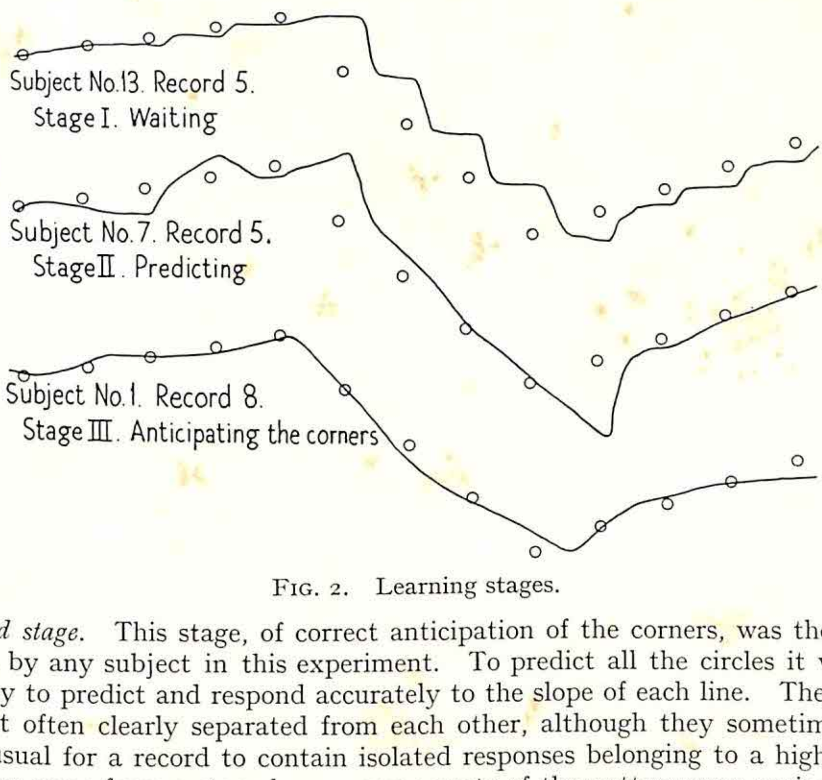


FIG. 2. Learning stages.

Third stage. This stage, of correct anticipation of the corners, was the highest reached by any subject in this experiment. To predict all the circles it would be necessary to predict and respond accurately to the slope of each line. These stages were not often clearly separated from each other, although they sometimes were; it was usual for a record to contain isolated responses belonging to a higher stage, as well as some from a stage lower—some parts of the pattern were easier to learn than other parts. But with very few exceptions one type of response clearly predominated. These stages, which are illustrated in Figure 2, indicate response first to single separate circles, then to parts of the pattern, and finally to the whole pattern.

The task given to the subjects was a fairly complex one, and the individual differences in achievement were considerable. This makes it difficult to state the results quantitatively. The intention is therefore to present them in a more general way, although no more than as hypotheses which may be tested in future experiments. A general picture of the results is given in Table I, in which subjects are ranked according to the number of correct predictions achieved in their best (which was

not necessarily the last) record. Seventeen of the twenty subjects were ranked in this way, as three performed the experiment in a way which prevented their records from being scored.

When subjects were thus ranked they seemed to fall into fairly well defined groups, the groups being distinguished in part by the stage of learning reached by the subjects. These groups will now be described. The appropriate figures are given in Table I.

TABLE I

	Performance			*Idea of pattern*					
Subject number	*Max. number of correct predictions*	*Number of dots hit*	*In record number*	*Pattern repeats*	*Consists of six parts*	*Direction alternates*	*Rhythm (slow–quick–quick)*	*4 dots in each line*	*W or S*
GROUP I									
1	96	48	8	R	—	R	R	R	—
2	95	49	7	R	R	R	R	R	S
3	93	37	8	R	—	R	—	R	—
4	92	56	6	R	R	R	R	R	S
5	88	17	8	R	R	R	R	R	—
GROUP II									
6	82	24	8	R	R	R	R	—	S, W
GROUP III									
7	77	12	8	—	—	—	R	—	W
8	76	14	7	R	R	R	R	—	S, W
9	75	29	4	—	—	R	—	—	—
10	75	20	5	—	—	R	—	R	W
GROUP IV									
11	71	24	8	R	R	R	R	—	—
12	69	40	7	—	—	R	—	R	—
13	69	22	8	R	R	R	R	—	—
14	68	20	6	—	—	R	—	—	W
15	67	13	8	—	—	R	—	—	—
16	50	15	8	—	—	R	—	R	—
17	27	12	7	—	—	—	—	—	—
GROUP V									
18	—	78	4	R	R	R	R	—	—
19	—	60	4	—	—	R	—	—	—
20	—	48	8	—	—	—	—	—	W

"R" indicates a correct idea.
In the last column "S" indicates a "step-like" distortion, and "W" a "wave-like" distortion.
Column 4 indicates which was the subject's best record; this being the one from which the scores in columns 2 and 3 are taken.

Group I. The first group contains five subjects whose scores varied from 88 to 96 out of the possible total of 96, and who distinguished themselves by predicting correctly a whole pattern at least once in the course of the experiment, and by the clarity of their idea of the pattern as revealed in their drawings. They all assumed that there would be a pattern, and proceeded to build up an idea of it, putting in from the beginning a mixture of responses; waiting, predicting, overshooting and anticipating. Their introspections included much information about the pattern, and they appeared to be both formulating the problem and looking for solutions. The final solution to the problem depended on knowing the points at which changes

of direction would occur. It involved therefore knowing and being able to make use of the information that there were four circles in each line. The way in which this information was obtained will be discussed later.

Example: Subject 1 (*Research Worker*). Began by predicting and overshooting, falling back on waiting when uncertain, or when expecting a change of direction. Began anticipating correctly and incorrectly in record 1, tried counting the circles in record 2 "to work out a rhythm." Learned line 3 in record 2, but had difficulty with lines 1 and 5. (Knew pairs of movements, but not how to join them.) She stated her difficulties: "I hit dots mainly on the quick ones, but sometimes move up too soon." She said she arrived at the solution to overshooting (the number of circles in each line) in record 5. Her drawing was correct except that it contained an extra peak. The learning curve rose steadily, having begun with 55 correct predictions in record 1.

Group II. The second group contains only one subject, who scored 82 correct predictions out of the possible total of 96, and who did not quite achieve one completely correct pattern. Her method appeared to be different in that she decided very quickly that the pattern of circles was a repeating one, and she worked all the time on this idea of the whole pattern.

Example: Subject 6 (*Research Worker*). In record 1 was waiting mostly and sometimes predicting and overshooting. Predictions increased in record 2, with some anticipations, mostly wrong. The number of correct anticipations increased gradually, mistakes occurring mostly in the same places. Introspections show that she was working on an idea of the whole pattern, conscious of repeating the same errors. Did not mention the number of circles (but when asked afterwards said: "They were more or less in units of four, I have just thought of that"). The drawing was mainly correct, but the number of circles wrong; and line 5 was drawn as wave-like. There were 28 correct predictions in record 1; then the learning curve rose fairly steeply.

Group III. In the third group there are four subjects, their maximum scores ranging from 75 to 77. They began well, but their learning curves rose little in the course of the experiment. Their introspections suggest that these subjects relied for learning mainly on "feel." With the exception of one subject who soon learnt the quicker parts of the pattern, there was little change in the nature of their responses; they remained throughout in the second stage of learning. Afterwards their idea of the pattern was much less clear than that of the subjects who ranked higher. Only one subject put in his drawing the main features of the pattern, and none put in the number of circles correctly, although one had stated the number correctly after his fifth record. These subjects did not appear to formulate the problem nor were they apparently worried by their repeated errors of overshooting. It is perhaps significant that all but one achieved a best record soon after the middle of the experiment, whereas most subjects were still learning on the seventh or eighth.

Example: Subject 10 (*Student*). Began predicting, sometimes wrongly, waiting occasionally, and anticipating changes of direction fairly often. His last record was much the same, with fewer anticipations. Knew the number of circles, stated after record 5: "I noticed they were in groups of four, so it will be easier to anticipate changes of direction." Did not formulate the difficulties; continued to overshoot. Introspections became more vague and general: "there is a subconscious patterning, I push the pencil one way without knowing why and sometimes hit a dot." Said afterwards was relying largely on feel. Drawing poor—including nine peaks of various sizes. Learning curve began with 66 correct predictions, fell slightly, then rose a little.

Group IV. With the lowest scores, ranging from 27 to 71, comes a group of seven subjects which is less homogeneous than the other groups, but which have in common the fact that they spent too long in the first learning stage, that of the waiting response. They either began with or fell back upon the waiting response, regarding it as a possible solution to the problem. It was the wrong solution and

could never be successful, but some subjects were convinced that if they could move fast enough when a circle appeared they would succeed in hitting it. In their introspections these subjects emphasized the difficulties of the task, or the dangers of incorrect prediction. Five of them were unable to draw the pattern, although the other two knew the shape of the pattern, but not the number of circles. Most of them were still learning at the end of the experiment.

Example: Subject 12 (*Research Worker*). Spent about half of record 1 waiting, then fell into predicting and waiting at the corners. This method continued until the end of the experiment, with only occasional anticipations, and occasional overshoots. Hit a large number of circles. Stressed the difficulties; worked out a "safe" method. After record 7 said: "It is necessary to be confident that one does one step properly and to continue to pay attention to that, rather than to try to leave that to be done automatically, and try to work out more complicated stages; if one does the latter, the former goes wrong." Knew the number of circles early; the drawing gave three series of four circles, on slightly different slopes, did not know the relationship between them. The learning curve began with 35 correct predictions, and rose unevenly.

Group V. The final group contains the three subjects whose records were not scorable because they used a different method. They concentrated mainly on the problem of hitting the circles and were highly successful in this respect.

Example: Subject 20 (*Fleet Air Arm*). Began with few predictions, often wrong. Then appeared to be waiting for each circle and shooting towards and past it when it appeared. Thus produced up and down movements, one for each circle. Was responding to the timing. After record 2 said: "It seems as if a certain number have the same interval between them, and if you timed the stroke, you could hit the dot." Continued in this method until the end; drawing very poor, including a horizontal line.

IV

Discussion

The results will be discussed in terms of the differences between groups. The discussion falls under four different, but related headings.

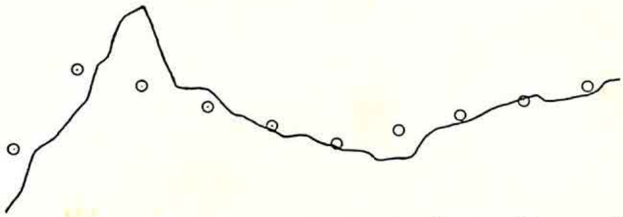
1. *The origin of the idea of the pattern.*

The nature of the task made it essential for the subjects to anticipate; it was not possible to succeed by responding to each circle separately. It was necessary to build up a correct idea of the whole pattern. The subjects realized this, and all but one mention the pattern as important. The results showed that, in general, a good drawing of the pattern corresponded with the capacity to predict it well, although there were exceptions to this which will be considered later.

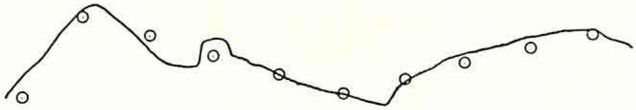
But, in addition to this, it was possible to relate the drawing of the pattern more closely with the motor record, by investigating the origin of distortions which occurred in the drawings. Half of the subjects introduced distortions into their drawings. These distortions are not readily explicable if considered only in relation to the actual pattern, but may be understood in relation to the mode of response.

One example of this was that five subjects (Nos. 6, 7, 8, 10 and 14) reproduced one or sometimes both of the slower parts of the pattern as a series of small waves. When their records were examined, it was found that their drawings corresponded well with their patterns of response. It is true that an uneven or wave-like response did occur in many records, including some where the subject drew the pattern as a series of straight lines. But when the single wave-like responses in the final record were counted, it was found that those subjects who reproduced the pattern as wave-like had produced far more of these irregular responses (between seven and eighteen) than the remainder, the number here varying from 0 to six only. Wave-

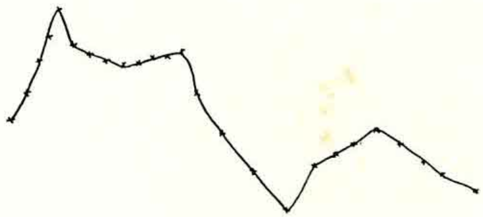
like responses of this kind can be shown to be built up in the course of predicting the pattern of circles, and so not to result solely from an assumption that the line is a wavy one. They occur in the flatter (and consequently from the subject's point of view, the slower) parts of the pattern, such as lines 1 and 2 or 4 and 5 (Fig. 1). When a subject overshoots at the end of line 3 or 6 (the quicker lines) he has to reverse the direction of his response on seeing the first circle in the next line, and often makes a rapid movement which takes him far beyond the second circle, and this causes him to reverse the direction of his response again, and so on. It is perhaps significant that three of these five subjects were members of Group III.



A. Subject No. 2. Record 2. (The last time he overshot in this part of the pattern).



B. Subject No. 2, Record 8. (His only error in this record).

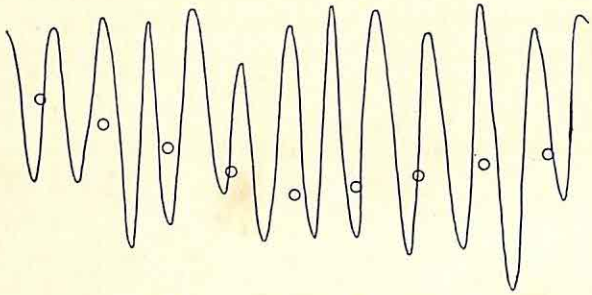


C. Subject No. 2. Drawing after completion of Experiment. (1st half only).

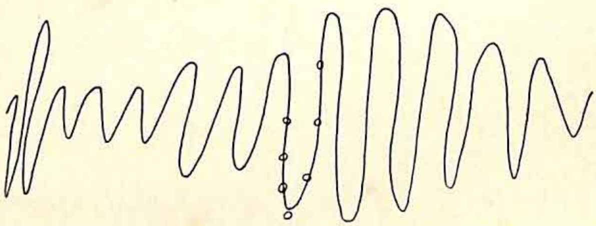
FIG. 3. Relation between nature of response and idea of pattern.

Again, six subjects (Nos. 2, 5, 6, 8, 11 and 15) revealed signs in their drawings of an error illustrated in Figure 3 from the records of subject 2. Line 1 or 4 in the pattern was begun with a rapid long movement which then flattened off (a "step-like" error in Table I). This is believed to be the effect of overshooting the previous lines so that the next movement in the opposite direction as far as the first circle is in fact larger than the subsequent movements. Something of this kind is described by subject 2 at the end of his second record: "Sometimes there has been a large step followed by smaller ones." He overshot very little after his third record, but the effect persists in his drawing. Although the overshooting which had given rise to this idea had long disappeared from the motor record, the idea of the pattern introduced a characteristic error into his responses (Fig. 3B). This was to begin line 4 with a quick downward movement at roughly the same speed as the previous quick line.

There were two atypical drawings which also illustrate this point. Subject 3, who was able to predict correctly, drew the pattern as a series of vertical rows of circles which suggest that the actual vertical response guided by the edge of the slit may have played a part in his reproduction of the pattern. A characteristic section of the response of subject 19 is given in Figure 4A and part of her drawing in Figure 4B.



A. Subject No. 19. Record 8.



B. Subject No. 19. Drawing after completion of Experiment.

FIG. 4. Relation between nature of response and idea of pattern.

Distortions of this kind provide evidence of a concept affecting the response. They suggest further that this concept has its origin in the whole situation, including the subject's motor responses; that it is in fact based on experience rather than on the objective situation alone.

It is true that these examples are exclusively distortions of the pattern. This does not imply that the response distorts the idea. It merely means that in this particular situation it is only the evidence provided by distortions which is unambiguous in elucidating the origin of the idea.

It is not suggested that the motor response alone provides the basis for the idea of the pattern. As has been mentioned, success in the motor performance does correlate with a clear idea of the pattern, but by no means completely. In spite of their capacity to predict the pattern without any errors, subjects 1 and 3 in particular were uncertain when they came to draw it; while subjects 11 and 13 reproduced the pattern adequately, although they were unable to predict it correctly, and, in addition, subject 18 produced a general idea of the pattern which was correct, although his mode of response alone (a rapid "scanning") could not provide any simple basis for this. This feature of the results will be discussed further in section 4.

The evidence suggests that the origin of the idea of the pattern is to be found only indirectly in the actual pattern. The actual pattern to which the subject responds may, from his point of view, be changed by his responses. The movements, although made in response to the actual circles, may sometimes explain distortions in the idea of the way in which they are placed.

Two similar experiments, using the same apparatus, but without the subject making an overt response, help to show what is meant by a response in this context. In the first, twenty subjects were instructed merely to watch the circles as they appeared in the slit, and to work out how they were arranged on the paper band. After seeing eight records they were asked to draw the pattern. No subject in this group made errors in his reproduction comparable to the "wave-like" and "step-like" errors of many subjects in the first experiment. A further group of ten subjects was given the same task, but in this case the instructions were to think of drawing a line through the circles. In this group two subjects made drawings which included both types of distortion. Both of these subjects said that they were following the circles with their eyes. "I am painting the line with my eyes," and "I was actually moving my eyes up and down to draw temperature charts."

2. *The subject's formulation of the problem.*

Success in this task depended on developing an idea of the pattern of circles as a whole, but the conditions made this difficult; the subjects were, in fact, presented with a problem, and their success depended to a considerable extent on how they formulated it. They were given no guidance, and they differed markedly in their approach.

Some subjects considered their performance in relation to the external situation, their errors and the completeness of their idea of the pattern or worked out different methods of tackling the problem, while others did none of these things. The point which emerges is that the most successful subjects, those in Groups I and II, were those who formulated the problem in terms of the task itself and looked for solutions, while the third group relied on "feel" or on some kind of "subconscious patterning" (in the words of subject 10) and did not try to formulate the problem. The fourth and least successful group contained those subjects who formulated the problem, but incorrectly, by considering objections or difficulties which were irrelevant (such as reaction time), or which made the solution more difficult (an unwillingness to predict because of the confusion produced by an incorrect prediction, or the adoption of the waiting method as a possible solution). It might be concluded from this that a tendency to formulate the problem represents a danger only, as in this case it resulted in seven subjects (Group IV) failing to some extent, while only six (Groups I and II) benefited from it. The difference is however a clear one: the successful subjects formulated the problem correctly, on the basis of their experience; they followed the evidence and tried to correct tendencies to respond at variance with the pattern. The unsuccessful subjects either failed to make this correction (Group III) or brought in extraneous concepts which prevented them from following the evidence (Group IV).

An incorrect formulation was not a purely intellectual error but could prevent the subject from responding in a way which might lead to a more correct formulation.

On account of this kind of incorrect hypothesis, a number of subjects in Group IV in particular continued to respond in a way which failed to emphasize the key points of the pattern (waiting response as in Fig. 2); these key points were emphasized for those subjects who made straightforward predictive responses in line with the pattern (as in the early record of subject 2, Fig. 3, or subject 7 in Fig. 2). It is apparent that the formulation of the problem was not exclusively an intellectual process, but depended also on the nature of the performance. The problem could not be formulated until the subject had some idea of the pattern; and his idea of the pattern depended in part at least on the level of his activity, so that the successful

subjects did in fact provide themselves with more information than the unsuccessful ones.

However, it is clear from the performance of subject 12, for example, that given enough time he might have constructed an idea of the pattern in a way which depended less on successful activity than on intellectual effort. The same is true of subject 18, although his approach was different. Again, this raises an apparent inconsistency which will be discussed in section 5.

In building up a useful idea of what to do in this kind of situation, it may be a help to formulate the problem in terms of the whole situation. It may be a danger, however, if this formulation is one-sided, or if it is accepted too early, and not changed in the light of further activity.

3. *The taking of a step from one stage of learning to a higher stage.*

Bryan and Harter (1897, 1899), in their study of the telegraphic language, developed the concept of learning stages; skill depends on the building up of a hierachy of habits; the elements must be learned first, and the advance to a higher stage depends on the elements becoming automatic; the learning of the elements is, however, only perfected at a higher stage; in each stage of learning the foundation is being laid for the advance to the higher stage, but the actual taking of this step depends on a strenuous and increasing effort. Book (1925) in his study of typewriting comes to similar conclusions.

In the present experiment the task is simpler than and rather different from telegraphy or typing, but here as well definite stages of learning can be distinguished. The stages exist as noticeably distinct modes of response in the graphic records, and so provide evidence of the idea of the pattern with which the subject was working. For example, the early record of subject 2 in Figure 3 shows that this subject understood the general trend of the pattern, but did not know when changes of direction would occur, while the waiting responses of subject 13, reproduced in Figure 2, show that he was probably uncertain even of the general trend.

There are three main factors which relate to the question of whether or not a subject would advance to a higher stage: (1) the nature of the responses, (2) the attitude of the subject towards his errors and (3) a tendency for the responses to become automatic.

The first factor has partly been dealt with in the preceding section, but the details need investigation. The crucial information in this case was the number of circles in each line; ability to utilize this information gave mastery over the key points of the pattern—the changes of direction. Three subjects in Group I counted the circles from the beginning and appeared to build an idea of the pattern round this information. The other two decided to count the circles as a solution to the problem of overshooting and as soon as they had counted correctly their performance improved. Of the subjects in the three other groups, only three mentioned the circles and these all counted them correctly. Subject 6 knew the number when asked afterwards, but had not thought of it before and had not made use of it. Subject 10 (Group III) said after record 5, "I noticed they were in groups of four, so it is easier to anticipate changes of direction," but did not go on to do so. The other subjects in Group III did not seem to be worried by their repeated errors of overshooting. In Group IV, subject 12 stated the number correctly after record 4, but this had no effect on his performance, and subject 16 grasped the number in record 7, confirming it in 8, but again made no use of the information.

This evidence is slight, but it raises an important point in connection with the relation between the motor and intellectual aspects of the task. It seems probable that the crucial information (which in this case could be grasped at any stage in the learning) could only be effectively utilized when the performance reached a certain level. In fact, this information was needed at a definite stage in the learning at the point when overshooting arose and was recognized as a problem.

The ability to advance from stage II to stage III therefore appeared to depend on two factors: the possession of a fairly coherent idea of the pattern, based on effective responses, and also a determination to complete this idea with the information required to eliminate errors.

This leads on to the second factor: the attitude of the subjects towards their errors. It is clear that subjects who became aware of making repeated errors, and tried to correct them, improved, while those who were either not worried by overshooting or were so cautious that they did not make mistakes, did not. Errors followed inevitably from attempts to vary responses away from the earlier and lower stages of learning. This point underlines that of Bryan and Harter, that the taking of a step towards a higher learning stage depends on strenuous effort on the part of the learner. A fairly universal feature of the learning is an increase in incorrect anticipations which is either simultaneous with or precedes a rise in the number of correct anticipations. But the subjects were then affected differently. Whereas those in Group I solved the problem presented by this increase in errors, those members of the other groups who tried anticipating and made mistakes gave up the attempt. As a result of their errors, they either regressed to an earlier stage of learning, or remained in the same stage. The cautiousness of some subjects in Group IV was caused by an unwillingness to make mistakes. Subject 14 fell back on the waiting response in his eighth record because of dissatisfaction with his performance.

It seems that taking a step towards a higher stage of learning involves the hazarding of solutions which, if not quite correct, often result in errors; at this point success depends on whether or not the subject is able or willing to persist, and the situation is complicated further by the fact that success also depends on discarding incorrect "solutions."

The third factor, also referred to by Bryan and Harter, concerns the necessity for responses to become automatic before the step to a higher stage is possible. In the present less complex task there is little direct evidence for this. One or two introspections are interesting in this connection. At the end of the experiment, subject 16 remarked that he had been so busy in the early part of the experiment that he had not thought of the possibility of there being a pattern; and after record 3, having stated his difficulties, subject 2 said: "I shall have more time now and skill, I shall be able to think more as I go along."

4. *Summary: the relation between the motor and intellectual aspects of the task.*

There is a complex relation between the motor and intellectual aspects of this task. Its exact nature needs further investigation, but a number of points have arisen which provide some guidance.

In particular, the graphic record reveals something of the intellectual processes, even if this needs to be supplemented by introspections and the drawings of the pattern. But the actual situation alone is not sufficient to explain the motor record except at a very early stage in the learning. The pattern of circles alone could not produce the wave-like distortion in some drawings; but it was sufficient to

provide for the wave-like responses which helped to determine the subject's perception, his idea of the pattern and so at a later stage could perpetuate the wave-like error.

It is important, therefore, to consider the subject's own view of the situation when his attention has been channelled by his activity; and in this task which requires predictive movements, his responses can reveal what he thinks the pattern is like. Whether objectively right or wrong they show what he is trying to do.

However, the experiment has raised some problems: it has been indicated that the intellectual activity is important in the learning; for example, in formulating the problem, in correcting errors and so on. At the same time it has been shown that this intellectual activity cannot be considered in separation from the motor activity: in that the subject's idea of the pattern is to some extent determined by it, and in that the development of the intellectual activity may depend very largely on the character of the motor response (as in seeking the final solution to the problem of hitting the circles).

But there were difficulties: in general, when the number of correct predictions was large, the drawing of the pattern was good, but there were cases where the drawing was a little better than the subject's predictions (e.g. subjects 11, 13 and 18). In addition, it was possible for subjects to predict the pattern correctly and afterwards make errors in their drawing.

The inconsistency implied by this could be resolved by considering the intellectual activity as an abstraction from the whole situation, including the motor responses, in fact, from the subject's total experience in this task. As has been shown, subjects tended to take a one-sided view of the situation, some concentrating mainly on the "feel" of their responses, and those whose responses were not completely correct, but who nevertheless gained a good idea of the pattern were clearly attending more to the actual pattern of circles. It is not implied by this that a change in the motor response must be followed inevitably by a change in the intellectual activity, in fact, the conditions in which this occurs are what need investigating.

There is evidence that the responses played a part in the structuring of the pattern, but the details are not known. This experiment could be improved by changing the pattern to one requiring less stereotyped responses. If the pattern were more complex and subjects were allowed to choose a path, it is likely that their drawings of the pattern would vary more. It would then be possible to compare this with their final path, and to work backwards through their records to find out how the idea arose.

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MISCELLANEA

AN APPARATUS PROVIDING SIGNALS AT "RANDOM" INTERVALS

BY

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WHEN designing apparatus for the study of skill, it is often difficult to decide how complex the task should be. On the one hand, equipment like the Cambridge Cockpit (Davis, 1946) sets a task which is complex and integrated, overall measures of performance are possible, but may be difficult to interpret in detail. On the other hand, attempts to break the task and its performance into simpler elements, such as are found in the McDougall Dotter or the Triple Tester, restrict the parameters of behaviour that can be studied with confidence. They produce tasks so artificial that it becomes difficult to relate the performance measured to everyday behaviour.

There are, however, some daily-life skills where an integrated behaviour is not possible, but in which discrete signals for action occur at irregular—apparently random—intervals of time. The occurrence of these signals can often be predicted with more or less accuracy, but sometimes even this is not possible. Such a task occurs in cotton spinning and winding, the signals then being breakages of the cotton thread. The apparatus which forms the subject of this note was built to enable this type of behaviour to be studied.

The apparatus presents a task in which signals occur with an *approximately* random distribution in time. There is virtually no repetition of the pattern of signals, the average rate of their occurrence being under the experimenter's control. The physical nature of the signals is not fixed, a number of different types being possible, and with some arrangements the subject can predict when they will occur.

A recent mathematical discussion by Cox and Smith (1953) has provided the basis for the design. The point made in this paper is that, if a number of sources each emit a periodic signal, the combined effect is a series of signals with an approximately exponential distribution of the time intervals between them. The limiting factors are that the periods must be slightly different and prime with respect to each other, not too different, and there must be a sufficient number of them. Whatever the number and length of the periods, the distribution of the time intervals between signals can be calculated, and the divergence from the exponential measured.

An apparatus using this principle consists of 16 small dials mounted in a 4×4 fashion. Each dial carries a pointer driven through a gear link by a single variable speed motor. This gear link is in each case such that every pointer revolves at a slightly different speed. Behind each dial face and mounted on the pointer spindle, is a cam which actuates a relay once every revolution. Subsequent events, electrical or mechanical, will depend on the type of signal required.

For example, the closing of the relay might stop the pointer at an indicated mark until the subject makes a correct response, the time of both events being recorded. Or by responding correctly before the pointer reached the indicated mark, the subject might prevent the pointer from stopping. Again the relevant time relations can be recorded easily. In both cases the behaviour of the subject would actively affect the nature of the display. Alternatively, closing the cam-actuated relay might leave the pointer unaffected, so that it continued its revolutions, but would

provide a signal at the moment of coincidence with the indicated mark. Again, the time of the signal and of the response to it could be recorded. The psychological problem in both of these cases would be anticipating which dial would provide the next signal. The dials as a display could be entirely dispensed with, and only the cam and relay element used to feed impulses either to 16 light or sound sources, or to a single one. In the last case there would be a calculable probability that more than one impulse at a time would claim the single-output source. Whichever the type of signal, its mean frequency could be controlled by varying the speed of the motor.

The 16-dial units are spatially interchangeable simply by interchanging pairs of gears, and of course units can be entirely detached from the effective display. By this means the display and the number of sources can be varied.

Some idea of the frequency distributions of the time intervals between signals when 16, 8 or 4 sources are used is given below for a mean signal speed of 60 signals per minute. This perhaps provides more useful data than a list of gear ratios on which the values below depend.

FREQUENCY DISTRIBUTION (PER CENT.) OF INTERVALS BETWEEN SIGNALS AT A MEAN SPEED OF 60 SIGNALS PER MINUTE

	Intervals (*units of* 0·25 *secs.*)								
	1	2	3	4	5	6	7	8	*Maximum value*
Exponential	22	17	14	10	9	6	5	3	inf.
16 sources	21	17	14	11	9	7	5	4	7·2 secs.
8 "	20	17	14	11	9	7	6	4	5·2 secs.
4 "	18	16	14	12	10	8	6	5	3·3 secs.

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Erratum

"In The Influence of Adaptation on Absolute Threshold Measurements for Olfactory Stimuli," by G. H. Cheesman and Stella Mayne.

Vol. V, No. 1, page 23, the fifth paragraph of Section II should read:—

The test stimulus was presented in a range of concentrations. Twelve 8 oz. standard reagent bottles were each labelled with a letter taken at random from the alphabet. Each test bottle was covered by a paper jacket to prevent the subjects observing the effervescence of the more concentrated solutions on shaking. Nine of the bottles were loaded each with 50 ml. of aqueous solution of the test stimulus material; the concentration of the stimulus in each bottle being different. The remaining three bottles in the series were loaded with 50 ml. of water and these were used as "catch" experiment bottles ("Vexierversuchsflaschen") as a check against guessing and as an indication of the reliability of the test. The range of concentrations was approximately equally distributed on either side of the expected threshold concentration as determined in a pilot run.

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FURTHER STUDIES IN THE PERCEPTION OF A CHANGING SHAPE

BY

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I

Koffka's hypothesis of "invariant relation" holding between tilt and perceived shape of an object is discussed from the standpoints both of logical tenability and of experimental findings.

After discussion of certain shortcomings in the method of investigation devised by Stavrianos, two experiments are suggested, the first, of a simple character, intended to follow out in practice the hypothetical conclusions of Koffka's argument, the second to test the hypothesis under conditions which attempt to ensure the preservation of the perceptual object throughout the arc of tilt. For this purpose subjects attempt to match two shapes, one of which is stationary whilst the other revolves at a constant speed. The comparison shapes cover the range of the possible arc of tilt.

The results of the first experiment, whilst appearing to contradict the requirements of the hypothesis, are by no means conclusive. The second experiment, though not absolutely conclusive, gives little support to Koffka. The main conclusion drawn is that there occur non-systematic variations in perceptual constancy throughout the arc of tilt. These are of a phasic character and are qualitatively different from results obtained by matching stationary shapes. There appears to be no simple function which would result in exhibiting a uniform constancy value for the entire arc of orientation.

Introduction

A previous paper (Langdon, 1951) described some phenomenal relationships noted in the course of investigating the general problem of shape constancy. The present paper deals more fully with certain other aspects of this question, in particular with the theory of invariance between tilt and perceived shape. This theory, originally proposed by Koffka (1935), has been the subject of considerable speculation and experiment.

It is now some twenty years since the work of Eissler (1933), and more since that of Thouless (1931), aroused the original controversy. More recently, interest in perceptual phenomena has veered away from the particular problems which gave rise to it, and a survey of present day literature reveals little in the form of experimental investigation which is likely to resolve the issue. As Vernon (1953a) has pointed out, the general trend of evidence from experimental studies has been to stress the divergencies rather than the similarities between the different perceptual phenomena. This has had the effect of displaying the Gestalt theories as oversimple formulations of complex problems. The tendency towards independent lines of development in perceptual theory, noted by the same writer (Vernon 1953b), has had the effect of diminishing the precise significance of detailed arguments about shape constancy and the shape-tilt relation, and of giving such discussions an air of outdated scholasticism.

Nonetheless, whatever may have been the fate of the general theories in which these problems were set, the problems themselves remain. Neither has more recent experimental and theoretical work suggested any plausible solutions. The abandonment of Gestalt propositions in favour of later formulations as regards general perceptual theory does not of itself result in the appearance of shape-tilt formulae as an inevitable corollary. Neither does it show that the shape-tilt relation is a pseudo-problem arising only from an incorrect theory. And this is hardly likely, since this relationship is a fact of experience based upon properties of the physical and visual world.

II

Theoretical Considerations

The postulation of a direct relationship between perceived size and distance for the case of size constancy has continued to receive confirmation. Thus the work of Holway and Boring (1941) has been confirmed by later studies and theoretical analysis by Gilinsky (1951), and by the work of Gibson (1947) and Ittleson (1951). Despite various disagreements by these authors on the significance of their work (cf. Brunswik, 1944; Joynson, 1949) and, particularly, on the order of importance of different perceptual cues, the general principle of the direct relation between distance and perceived size seems generally agreed upon.

Acceptance of this principle would, not surprisingly, lead to the expectation of an analogous relation between tilt and perceived shape. Such an application is entirely comprehensible since shape is defined in terms of size, and is considered as an extension of it. Thus the O.E.D. (1914) defines shape as "That quality of a material object or a geometrical figure which depends on constant relations of position and proportionate distance among all the points composing its outline or external surface."

Koffka, in discussing shape constancy, assumes the application of the size-distance relation, and he develops these assumptions in his theory of the "invariant relation." This theory he states in the course of criticising the work of Thouless and Eissler. Thouless, as is well-known, had found variations in shape constancy over the arc of possible tilt (line of regard to frontal parallel), calculating constancy as a standard index by means of the formula Log P–Log S/Log R–Log S; the indices for various angles to the line of regard were as follows:—

7°	10°	20°	30°	45°	65°	90°
·28	·41	·33	·32	·16	·1	/

Thus here, constancy is variable, being greatest when the angle is smallest. Eissler, employing a slightly different technique, had also obtained varying amounts of constancy through the arc of tilt, though his results appear to confiict with those of Thouless. The variation, which he termed "relative constancy," was in the opposite direction to that observed by Thouless. But the "absolute constancy," that is, relative constancy treated by a "transformation function" increased with angle of tilt. The transformation ratio (Verarbeits Funktion) $= A = \frac{S-P.}{P}$

Thouless had accounted for the variations in constancy by the hypothesis of "phenomenal regression," which stated that, where the perceptual indications were in conflict, there was a tendency for the resultant perception to "regress" to the character of the real object. Eissler, for his part, suggested that variations arose out of the subjects' attempts to compensate for the growing disparity visible between

the "real" and the "apparent" shape through the increasing tilt-angle. An increase in constancy is the outcome of "overcompensation" by the subject. The same suggestion is made by Stavrianos (1945) and, in the case of whiteness constancy, by Hsia (1943). It will be seen that although they express it differently, Thouless and Eissler are saying very much the same thing, since both look upon variations as outcomes of conflict between different perceptual cues.

Koffka commences his discussion (pp. 227ff.) by suggesting that the true cause of variations in scores is an outcome of the formula. For, says Koffka, the formula assumes that the tilted shape (a circle) is always seen as such, so that the range of constancies is always calculated in terms of the frontal-parallel as the reference plane. But if this assumption failed to hold, variations in constancy could arise from subjects perceiving the shape not as a circle, but as an ellipse with a minor axis somewhere between the axis of the standard figure and that of the tilted comparison circle.

The assumption of an invariant relation between tilt and perceived shape demands as a corollary that a measure of shape constancy for an object perceived under constant conditions should remain the same through the arc of tilt. Moreover, such a measure must be "unique," i.e., it must refer only to constancy of a given shape at a given orientation, and it must show that the true shape was perceived with a given degree of perceptual constancy. This, it is submitted, the constancy formula fails to do. Koffka gives a suppositious example of an ellipse with minor axis 15 cm., major axis 20 cm., seen at an angle of 45° from the line of regard. The retinal image of such a shape equals that produced by a frontal-parallel ellipse with a minor axis of 10·7 cm. But it also equals the retinal image of a circle seen at an angle of 15° 13 in. to the line of regard. The formula ignores shapes lying between these two, though there is no *a priori* reason why it should do so.

Since it is already laid down that "two proximal stimuli, if more than liminally different, cannot produce exactly the same effect" (p. 228), then the two ellipses which appear to be of similar shape must be examples of "pure orientation," and variations in shape constancy through the orientational arc are due to shortcomings in the formula which conceal the tilt-shape relationship.

This is a much condensed statement of the gist of Koffka's argument, and it appears to be the main one on which his adducement of an invariant shape tilt relation appears to rest, together with the assumed analogy of the size/distance relation. At the same time it should, in fairness to Koffka, be said that he qualifies this expectation with the proviso (p. 233) that the invariant may depend upon total sets of conditions and need not be identical under all conditions.

It is first of all necessary to note two points arising from this argument before passing on to consider any scientific evidence in support of it. A careful study of the text will reveal that Koffka is arguing by means of an undemonstrated converse. Thus it can be a valid premise that there are unexplained variations in shape constancy computed by means of the Brunswik-Thouless formula, and that the formula fails to state explicitly the determinants (in their respective terms of shape and inclination) governing the perceptual process. But this is not sufficient to demonstrate that such variations are due solely to the shortcomings of the formula, and hence the converse, that a correct formula would disclose an invariant relation. Such a relation is not the logical product of the discussion, but an entirely new postulate, independent of it.

Secondly, from having demonstrated by means of a suppositious case the possibility of the two different shapes having been perceived as similar, or conversely, of the standard being perceived as other than it is, Koffka proceeds as if this example

were a real one. For the only evidence offered in support of this contention is isolated instances from Thouless's and Eissler's studies. And these, as shown above, do not prove what they are assumed to prove.

For if the subject saw the "real" shape as other than it was, or other than he saw it at another time, this would not in any way afford evidence of an "invariant relation" between perceived shape and inclination. The only evidence which could suggest such a relation would be that which showed that a different constancy value was associated with a different perception of tilt. Nowhere, however, does Koffka offer such evidence.

It would seem then, that on logical grounds alone the argument for this relationship, as presented by Koffka, is not very convincing; nor is the case much helped when experimental evidence is examined.

The main study prompted by this hypothesis was that of Stavrianos (1945). Her subjects attempted to estimate the degree of tilt by their perception of shape. Her results are unfortunately not in a form convenient for direct presentation. In summary, subjects attempted both estimates of shape and explicit judgments of inclination at four selected angles of tilt (15°, 30°, 45°, 55°). This occurred under conditions of binocular normal vision, binocular reduction-tube vision, monocular vision, and monocular reduction-tube vision. Paired judgments for shape and tilt failed to show any significant correlation. At the same time, the comparison of mean standard error under decreasingly favourable conditions through reduced orientation cues showed a corresponding decrease in accuracy of estimated tilt. This was not accompanied, however, by a corresponding decrease in accuracy of estimated shape. The *accuracy* of estimated shape remained surprisingly high for all conditions, whilst variations in the Brunswik-Thouless ratio did not occur systematically as a function of presented angle. On the other hand, an approximate relation of invariance for perceived shape and angle of tilt was found, under the monocular condition, for some observers.

From these experiments Stavrianos concluded that failure to demonstrate a shape-tilt relation was due to the difference between the observer's adjustment of tilt (or shape) and the tilt (or shape) which he "took into account" or "registered" when making this adjustment. Hence the implication remains that a different attitude or reaction is involved in perception of tilt, as distinct from the perception of shape.

It might perhaps be thought that since perception of shape and perception of tilt involve different reactions (the subject directing his attention to different ends) an invariant relation between these factors is somewhat improbable. This would be an unwarranted conclusion, however, since, although the experiments aim at correlating such factors and fail to do so, Koffka's hypothesis does not in fact require such a correlation for its confirmation. The hypothesis of invariance merely states that an invariant relation exists between the perception of a shape and of its degree of tilt, not between the perception of a shape and an *estimate* of its degree of tilt. And Stavrianos is herself clearly aware of the superogatory nature of the task before her demonstration, since she states that in failing to uphold the hypothesis of invariant relation the experiments may have offered too severe a test of it.

Although the elusive concept put forward by Koffka appears to have escaped the grasp of Stavrianos' investigation, it is not impossible to conceive of techniques which might go some way to confirm or confute it. And at least two ways of doing this are suggested by Koffka's original discussion.

In the first place, his suppositious case may be treated as an actual one. Two shapes, one a circle, the other an ellipse, must be matched with some third shape

such as another ellipse. They must yield similar constancies and simultaneously appear similar in order to confirm his criticism of Thouless's formula. In the second place, the fate of constancy needs to be investigated throughout the arc of inclination, under conditions which make it a reasonable assumption that the shape undergoing tilt *continues* to be perceived as physically unchanged.

Under these circumstances, if constancy throughout the arc of tilt were to remain constant, the formula might be said to disclose an invariant relation. Alternatively, if changes in constancy occurred corresponding to some simple function of the tilt-arc (i.e. a geometrical function such as the sine of the angle), such a relation might be said to be exhibited by a suitably amplified formula. But if it were not found possible to assign any simple function whatever to calculate a standard constancy, the likelihood of an invariant relation would appear remote.

Before concluding this discussion a final point must be noted. The technique employed in all these studies of shape constancy is one which necessarily precludes the attainment of complete constancy, and which, moreover, cannot preserve absolutely identical conditions throughout the arc of tilt. For in all such experiments the comparison shape is not physically changed; a flat surface is simply rotated upon an axis. The more remote implications of this fact are discussed below (p. 104). For the moment it is sufficient to note one obvious outcome. If a shape such as the circle is compared with an ellipse which is a projection of the circle tilted 10° from the frontal-parallel plane, the possible range of constancy is some 80°, whereas a similar disc turned for comparison with an 80° ellipse can only be turned another 10° before disappearing altogether. Thus the actual conditions of constancy are in part dependent upon the degree of tilt. And neither treatment with nonlinear functions, nor the employment of "transformation functions" in the manner of Eissler can overcome these difficulties. For this reason, if for no other, it would seem that any attempt to develop a general function for shape constancy analogous to the size/distance relation can only be of limited value, *even were the hypothesis of invariance upheld.*

III

Experiment I: Method of Investigation

In the above discussion two possible lines of investigation were suggested, the first of which aimed at reproducing in actuality the suppositious example given by Koffka. The means of achieving this were extremely simple.

Three flat shapes were mounted upon a platform by means of vertical rods, these last forming the axes of the shapes, their lower ends passing through the base of the platform. Within the base protractor scales were mounted, and pointers attached to the rods travelled over their surfaces. When the pointers were moved, the shapes rotated on their vertical axes.

These shapes were a circle and two ellipses. The circle was 20 cm in diameter and was placed on the left-hand side of the platform. The centrally placed ellipse was 10·7 cm in minor axis, whilst the ellipse on the right-hand side had a minor axis of 15 cm. All the shapes were made from matt card optically blacked to minimize the total range of albedo and thus lessen the surface cues to tilt-angle. The three shapes were separated by a distance of 30 cm between their centres, and the apparatus was covered by a screen which could be slid from side to side, revealing in turn either the centre and left-hand shapes, or the centre and right-hand ones.

The subject was seated in a chair equipped with head and chin rest, directly before the central shape. All shapes were placed at eye-level. He viewed the apparatus from three different distances in turn, namely 6, 4, and 2 metres, in a large room lit evenly throughout by means by six 120-watt fluorescent lamps.

IV

Part I DESCRIPTION OF EXPERIMENT I

Whilst the subject was being seated, the apparatus remained covered with the screen, upon which was a large central fixation X to enable the chair to be adjusted to the correct position. When the subject was ready, the screen was moved to one side, revealing the circle (left) and the small ellipse (centre, appearing right), but covering the third ellipse. Under the subject's instructions the experimenter adjusted the movable circle, step by step, until the subject indicated a match between the two shapes. After noting the angle of inclination shown by the pointer, the experimenter turned the circle to the extreme position (line of regard) and worked back step by step until the subject once more declared the two shapes to be most similar.

The circle was returned to the original frontal-parallel position, and the screen moved to reveal the ellipse on the right-hand side and the central standard ellipse, which now appeared on the left whilst the screen covered the circle. The procedure described was then repeated. Finally, the experiment was repeated at the other distances.

Part II

After these observations had been made, the screen was moved to cover the entire set-up. The experimenter then removed the standard ellipse from the central position and brought the two outer rotatable shapes to positions corresponding to the means of their former settings. The screen was removed and the subject asked whether the shapes appeared similar, if they matched one another. Finally the standard ellipse was replaced and the subject asked to comment on the appearance of the entire collection. Were the shapes similar in appearance, did they match, etc.? In conclusion, subjects were asked for general introspective comments and reflections.

Part III

With the three shapes in their final positions the lights were extinguished, a small light source was exposed behind the platform and a white screen raised behind the apparatus. This had the effect of exhibiting the shapes as silhouettes against a dimly illuminated ground in an almost totally dark room. The subject was then asked if the shapes appeared to match one another. If the answer was no, then the two variable shapes were rotated step by step until subjects declared them most similar.

For the observations of Part I six subjects were used, none of them psychologists and all having perfect sight without spectacles. Some subjects may have read of constancy phenomena, but none knew any formal psychology or the purpose of the experiment. All were warned against feeling that they were "on trial" or that test results depended upon or implicated their mental powers or abilities. In Parts II and III only one distance was used for each subject in order to shorten a rather fatiguing session, the range being covered by different subjects. Subjects were told they might arrive at their estimation of the appearance of the forms by any means they chose, but not to attempt to move their heads or try squinting or screwing up their eyes. For all observations full binocular vision was used.

V

EXPERIMENT I: RESULTS

Table I gives the scores recorded by six subjects for the circle and variable ellipse, together with constancy values rendered by the simple Thouless formula. In the case of Part III, the settings arrived at varied so little from the calculated values for retinal image (projective values) that it is hardly worth while to record them in detail. Under these conditions all subjects recorded stimulus matches and showed little personal variation.

Match scores for two shapes are recorded at three distances. Angles and Thouless ratios are each the mean of three trials for each subject.

In Part II, subjects inspected the shapes set at the positions in which they had already been separately matched, but with the central standard ellipse removed.

All subjects denied any resemblance between the appearance of the two shapes. Most thought the circle appeared wider than the ellipse. When the central standard ellipse was replaced, all declared none of the shapes similar, but most thought the standard narrower than the other two shapes, rating them in order of circle (widest), variable ellipse, standard ellipse (narrowest). All subjects were definite that the widest appearing shape was actually a circle. Two subjects stated that they saw the shapes as different but could not specify clearly what the differences appeared to be.

TABLE I

Sex	*Subject*	*Shapes*	*Distance* *6M*	*4M*	*2M*	*Mean* *6M*	*Thouless* *4M*	*Values* *2M*
F.	A.M.	Circle	54·3	56·6	60·0	·2	·26	·34
		Ellipse	39·2	39·0	40·2	·1	·1	·13
M.	L.M.	Circle	53·0	55·5	44·7	·18	·23	—
		Ellipse	49·0	53·0	54·3	·28	·35	·37
M.	E.L.	Circle	61·0	63·3	66·0	·36	·4	·47
		Ellipse	51·5	53·3	58·0	·32	·36	·44
M.	G.H.	Circle	56·0	55·5	55·5	·25	·23	·23
		Ellipse	53·0	47·0	51·0	·35	·25	·31
F.	H.B.	Circle	57·5	60·0	65·0	·28	·34	·44
		Ellipse	52·5	53·0	60·0	·34	·35	·47
M.	D.M.	Circle	55·0	56·5	58·2	·25	·29	·35
		Ellipse	46·0	50·0	54·3	·22	·26	·29
Pooled Mean:								
		Circle	56·1	58·0	58·2	·25	·29	·35
		Ellipse	48·5	49·2	53·0	·26	·29	·35

Subjects 6. N = 228.

In Part III, all subjects declared that the set positions did not constitute a matched set of shapes. The positions at which they agreed on a genuine match of shapes were, as reported, a distribution about the point of stimulus equality.

VI

Experiment I: Discussion

The results of this experiment are not by themselves conclusive, since, although the pooled mean given in terms of Thouless ratio shows similar constancies for two different shapes (thus appearing to support the formula as against Koffka), the effect is purely adventitious. Thus, if the modified Brunswik formula is employed, or the pooled mean constancy is calculated direct from the pooled mean scores, differences in these constancy values arise. Moreover, inspection of individual scores in terms of angular setting and the mean Thouless values for each subject shows great differences in the way the two shapes were perceived. In addition, although a rough estimate of significance carried out by the writer failed to show significant differences, this is mainly because of the wide differences between individuals rather than because of any uncertainty of estimates.

But results from Parts II and III, as also comments and introspections of subjects, showed clearly a recognition of differences in the appearance of the two shapes, although their implicit registration of this in terms of angular placing of the shapes seems wayward and variable. If the results of Part I are contrasted with those of Parts II and III it would seem that each shape is regarded as a form "in its own right," the constancy value recording the reaction to this particular situation. Thus another shape may (in isolation) be matched in such a way as to make the second choice appear incompatible with the first when these two are brought into juxtaposition.

As Stavrianos found in attempting to relate implicit orientation of tilted shapes, it is very difficult to try to guess what a particular match of "apparent shape" represents in terms of the notion of "tilt" or orientation possessed by the subject at that moment. Yet there does seem some degree of reliability in subject's choices; again and again the subject makes the same setting. Variations in subsequent settings are very small (order of $30'' - 1{\cdot}0^\circ$) and space error is small (around 2°).

In introspection, subjects are far from sure of themselves. Their apparent reliability in accuracy of setting (measured by repeated observation) contrasts strangely with their diffidence in discussion. Most stated they were not sure about the correctness of their estimates, though giving confident commands of "stop" "that's enough" to the experimenter in the actual course of observations.

To summarize briefly the results of this experiment, it would seem that, whilst far from conclusive, they are not those required to support Koffka's criticisms of Thouless and Eissler. Whereas his argument demands that the formula give different constancy values whilst the subjects see similar phenomena, production of the example appears to indicate that subjects see different phenomena (and become aware of this) whilst constancy values vary in an unsystematic fashion. Insofar as there are differences in constancy, it is not beyond possibility that different shapes have different potentialities in evoking perceptual reactions on the part of the observer.

An incidental outcome of the experiment is the systematic variation of constancy with distance in the direction of greater constancy with shorter distance. The effect may be due to a number of causes such as increased perception of micro-structure when nearer the object, increased angular separation (cf. Joynson, 1949b) or increasing disparity of the two uniocular stimulus patterns. All or any of these factors tend to produce more effective criteria for the perception of the true shape of the object.

VII

Aim of Experiment II

In taking up the second of the two lines of enquiry suggested in the theoretical discussion, the shortcomings of this first test of Koffka's hypothesis will, it is hoped, be overcome. It should be clear that the first experiment represented only a rough "realization" of what remained purely hypothetical in the original discussion and cannot be taken very seriously as evidence.

When a shape is turned so that its full projective outline may not be seen by an observer (i.e. away from his frontal-parallel plane), then what does the seen shape relate to? It is Koffka's contention that perceived shape, under optimal perceptual conditions, relates to the frontal-parallel shape, i.e. to a shape seen at full constancy. A departure from full constancy is only possible through the failure to achieve optimal conditions of perception. For a given level of perceptual conditions, the constancy should remain constant throughout the arc of tilt for a given shape.

However, there has so far been no way of clearly establishing that there is some definitive shape which continues its, so to say, perceptual existence unchanged so that, at whatever angle it be viewed, it is a reasonable assumption that it relates to the shape it presents when turned to frontal-parallel. Thus in Experiment I there is no way of establishing unequivocally that matches of subjects did relate to the specific circle and ellipse presented. The simple constancy formula—the only means suggested so far, whereby perceptual judgments of different things may be compared among one another—does not give a definite answer.

So far then, despite contrary indications, Koffka may yet be correct in maintaining that variations are due to the tilted shape being seen as other than it in fact is, without the formula being able to indicate what this difference amounts to.

Thus what is required is a means of ensuring that the experimenter knows what actual shape the subject is perceiving throughout its various changes, and guaranteeing that this "notion" remains constant.

In the normal type of constancy experiment, the comparison of a pair of shapes is carried out step by step. Between each presentation there may be concealment of the stimuli. Whether or not this is done, there will occur some forty or fifty adjustments during which there is no return to the initial frontal-parallel position, whilst the whole operation may take considerable time.

It might well be argued that the final estimate has a direct relation only to the shape then seen and little relation to the object producing it. That is, it might be produced by some other object. In this case the estimated constancy is not inherently unique, and it is a matter of dubious theory that it is so.

It is therefore essential that the technique employed should endeavour to preserve the "object character" of the changing stimulus whilst at the same time it ensures that only present conditions of perception are the immediate cues on which the subject can base his estimate. It could then be assumed that the subject perceives the "true shape" and not the shape as "other than it is."

A second point which may be noted in passing is the tendency for the subject's estimate to be affected by extraneous factors such as the time taken by the experiment. Evidence from earlier experiments suggests that step by step setting of the stimulus can result in significantly different outcomes according to whether adjustments are made regular and frequent, or irregular and infrequent. The effect may be generalized in the direction of raised constancy for regular and frequent setting of the stimulus, and vice versa. Hence it was thought that the technique to be described would result in the lessening of variations from these causes.

The main aim of the experiment is, then, to explore the fate of constancy through the arc of tilt under closely controlled conditions, whilst ensuring that the continuity, substantiality or "object character" of the changing stimulus is stressed by rotating the standard shape continuously and having the subject record his estimates "en passant."

VIII

Method of Experiment II

A small D.C. electric motor was mounted on a platform together with relay switching mechanism. The relays enabled the motor to be reversed automatically at any desired point during the revolution of its shaft. On the final shaft of the motor, which protruded vertically from the platform, was mounted a circular card 25 cm. in diameter with a matt-black surface. By means of the mechanism described, this object would rotate from frontal-parallel to the line of regard and return to its initial position. The rate of sweep for one excursion was 10 secs.

Beneath the platform a moving pointer arm which actuated the reversing mechanism

held at its extremity a small solenoid relay in which was mounted the element of a ball-point pen. Immediately beneath the pen point lay the perimeter of a large perspex protractor. The solenoid was connected to a battery and a push button held by the subject. When the button was pressed the solenoid caused the pen to descend, thus touching the surface of the protractor. At the completion of each 90° arc the solenoid assembly was caused automatically to move $\frac{1}{8}$ in. along the radial arm. In this way successive scores could not fall one on the other, but extended radially across the scale of the protractor.

The apparatus was located 6 metres distant from the subject and at the same distance was a similar looking platform carrying the comparison shape. The two stimuli were separated by an included angle of 10° and on a great arc. Thus both were exactly "straight ahead" for the subject, yet could not be fixated simultaneously.

The comparison shapes were eight ellipses, each of which was a projection of the circle in steps of 10°, from 10° to 80° through the arc of tilt. Thus constancy judgments might be obtained for the range of possible tilt from frontal-parallel to line of regard. The subject sat comfortably in a chair with the objects at eye-level. No provision was made for securing his head. When the subject's button was pressed, a small red lamp (invisible to the subject) glowed, thus informing the experimenter that the subject had made a response. The apparatus was arranged so that four possible movements of the shape could be studied, tilting from centre to extreme left, left to centre, centre to extreme right, and right to centre.

IX

Part I

Description of Experiment II

New subjects who had not taken part in Experiment I were used in this experiment. The subject was brought into the room whilst the apparatus remained covered with a screen. When the screen was removed two black matt shapes, one a circle the other an ellipse, were visible against a neutral buff background. Without giving any instructions or mentioning the purpose of the experiment, the experimenter set the mechanism in motion, introducing it merely as a phenomenon which the subject was invited to watch and comment upon.

After the subject had watched the rotating shape for some moments, he was asked for any observations upon what he had seen. These having been noted, the subject was given the small hand push-button and told what to do, namely, press it when he felt the two shapes to be most similar.

He was warned against attempting to make every possible match whenever the point of similarity appeared, but only to do so if and when he felt sure of himself. The subject was asked not to make head movements, nor to squint, close or screw up his eyes in order to make matches, but to use his normal vision. After recording four attempted matches in each direction of tilt, the apparatus was stopped with the shape at frontal-parallel, the scores were noted, the perspex scale was wiped clean, and the fixed ellipse was removed and replaced by another.

The series of ellipses was presented in randomized order with a fixed succession, and beginning with a different ellipse for each subject. Thus for each subject there was a different shape as his "first experience," and from this set of "first experiences" a set of values could be obtained and compared with the pooled mean for the group. No significant difference was detected by comparison of such scores.

Upon the conclusion of this experiment (taking about 45 mins.) the series of shapes was explored once more, step by step, using hand setting. This time each subject took four shapes (20°, 40°, 60°, 80°) (10°, 30°, 50°, 70°), partly due to the lesser importance of this part of the experiment, but also because the latter procedure was lengthy and fatiguing for the subject.

Part II

In the second part of the experiment the black matt cards were replaced by wire outlines, thus removing possible illumination and textural cues to perception of tilt. The experiment was repeated identically with that described above, excepting that the experience was of necessity no longer a novel one.

All subjects taking part in these experiments had perfect vision or suitable correction by spectacles. In the case of these latter, only those suffering from simple visual defects

(no astigmatics) were admitted. Only two subjects had taken part before in psychological experiments. Of the ten subjects selected five were women and five were men. None were psychology students. Six were university students, four were workers or housewives.

X

Results

These are listed below (Tables II–VI and Graph I). Before discussing these, the general comments of the subjects will be dealt with. It must be remembered that subjects never saw the apparatus at close hand on first entering the room. When they did see it, it was some 6 metres distant, at eye-level and on a great arc of their saggital line. It will therefore be appreciated that the sole experience, in terms of sensory stimulation, was that of an ellipse (left) and a circle (right) which declined through various elliptical shapes to a straight line and returned once more to its original form.

TABLE II (a)

Solid Shapes Mechanically Rotated

80° In	80° Out	70° In	70° Out	60° In	60° Out	50° In	50° Out	40° In	40° Out	30° In	30° Out	20° In	20° Out	10° In	10° Out	Subject
·55	·5	·1	·05	·03	·03	·08	·08	·22	·16	·13	·14	·14	·11	·12	·19	1
·7	·7	·45	·57	·27	·3	·33	·35	·2	·32	·22	·32	·33	·33	·31	·35	2
·7	·7	·37	·55	·2	·4	·22	·37	·24	·26	·23	·18	·24	·21	·24	·22	3
·4	·35	·2	·35	·11	·33	·14	·35	·06	·34	·06	·33	·1	·1	·15	·23	4
·7	·7	·2	·25	·26	·26	·4	·4	·34	·22	·3	·23	·12	·16	·09	·08	5
·75	·8	·35	·2	·28	·25	·17	·25	·22	·29	·1	·1	·11	·08	·25	·25	6
·6	·7	·3	·2	·3	·33	·2	·12	·1	·22	·19	·28	·2	·2	·14	·15	7
·7	·7	·55	·5	·27	·34	·17	·32	·26	·28	·28	·23	·24	·24	·22	·2	8
·6	·6	·4	·45	·3	·28	·35	·37	·26	·24	·2	·22	·24	·29	·27	·29	9
·7	·75	·45	·47	·26	·32	·34	·35	·3	·27	·22	·25	·18	·2	·15	·17	10
·65	·65	·34	·33	·23	·28	·24	·3	·22	·26	·19	·23	·19	·19	·2	·22	Mean (In–Out)
·65		·335		·255		·27		·24		·21		·19		·21		Mean (Combined)

Constancies given in Thouless values for every 10° of arc, moving in two directions. Subjects—10. N–1280.

Scanty though this sensory information was, all subjects stated that they "*saw* a circle turning round." At a later stage an attempt was made to draw subjects into a discussion of their experience. All efforts to encourage them to entertain doubts as to the unambiguous character of their perception were unavailing. It was eventually pointed out to them that they could not "see" this but only infer it (it should be self-evident that the appearance may be produced without any rotatory motion, as in the case of Lissajous figures), but the general reaction was that the experimenter was indulging in a linguistic quibble. On the other hand, at a later stage in the experiment proper, almost all subjects stated that suddenly they no longer saw a "circle going round" but a two dimensional changing shape.

During the preliminaries most subjects described the phenomenon as interesting to watch but commonplace and unremarkable. Most expressed preference for watching the wire outline rather than the solid form. During the course of the experiment there would always come a point at which the subject would declare that he now saw a new appearance. Various terms were used to describe this: "A circle being squeezed in and out"; "A football bladder"; "The sort of thing a dentist uses when he gives you gas"; "An orange being squashed," etc. All subjects

resorted to metaphorical terms of visual imagery. One subject, after giving responses for some time, suddenly described such an alternation of experience and then stated that she "could not do the experiment properly unless I see the shape being squeezed." Seeing the circle turning round then appeared to "make her go wrong." Inspection of her scores, however, failed to reveal any systematic differences

TABLE III (a)

Wire Outline Shapes Mechanically Rotated

80°		70°		60°		50°		40°		30°		20°		10°		
In	Out	In	Out	In	Out	In	Out	In	Out	In	Out	In	Out	In	Out	Subject
·7	·65	·35	·42	·14	·22	·21	·25	·34	·37	·27	·29	·19	·15	·13	·17	1
·75	·75	·57	·6	·42	·47	·25	·27	·4	·42	·33	·39	·27	·27	·2	·24	2
·42	·4	·27	·33	·21	·27	·19	·17	·25	·27	·26	·29	·15	·22	·15	·19	3
·75	·75	·44	·48	·29	·37	·22	·25	·21	·27	·2	·23	·21	·18	·19	·2	4
·8	·8	·3	·37	·28	·27	·4	·47	·32	·3	·32	·27	·12	·18	·1	·11	5
·75	·8	·42	·31	·3	·27	·22	·29	·25	·31	·18	·14	·11	·14	·25	·27	6
·7	·7	·44	·32	·37	·39	·27	·22	·17	·2	·2	·27	·22	·23	·16	·17	7
·7	·7	·57	·62	·33	·37	·28	·35	·3	·32	·29	·3	·25	·25	·26	·25	8
·7	·7	·5	·52	·37	·3	·35	·35	·27	·28	·24	·25	·24	·21	·26	·28	9
·7	·7	·5	·55	·3	·33	·4	·41	·33	·32	·25	·28	·2	·22	·19	·19	10
·69	·7	·44	·45	·30	·33	·28	·3	·28	·31	·25	·27	·2	·21	·19	·21	Mean (In–Out)
·7		·45		·32		·29		·3		·26		·21		·20		Mean (Combined)

Constancies given in Thouless values for every 10° of arc, moving in two directions. Subjects—10. N = 1280.

TABLE II (b)

Solid Shapes Mechanically Rotated

80°	70°	60°	50°	40°	30°	20°	10°
1·0	1·66	2·23	1·62	2·1	2·55	3·03	3·5
1·0	2·42	2·12	4·6	4·2	3·16	2·87	2·55
0	2·7	3·42	4·1	2·55	3·27	2·94	1·9
1·9	2·5	3·87	5·1	7·35	8·54	2·22	3·87
1·0	2·06	2·12	1·32	4·43	4·12	2·92	3·16
1·0	2·5	2·45	2·22	3·0	4·31	5·1	3·2
1·62	2·64	1·18	3·24	4·18	3·32	5·66	3·0
1·0	1·27	1·63	3·32	2·0	2·56	2·23	1·8
1·0	1·8	2·16	3·11	3·5	4·23	2·7	2·88
1·0	2·2	2·8	2·32	3·76	4·54	3·74	3·33
1·05	2·18	2·4	3·1	3·71	4·05	3·35	2·92

σ of mean space errors for each subject. Pooled mean for group.

in response to either type of phenomenon, and this may be generalized for all subjects in that variations of experience do not appear to affect their matchings of the shapes, neither do their volunteered opinions on how they think they are responding: (typical excerpts:—(subject) "don't take any notice of that one (a score), I was a bit late then," or "I'm not doing so well now," or "the narrow ellipses were easiest, these (oblate) are harder," etc., etc.).

There may be perceptual differences in these subjective states of mind, but they do not seem to correspond in any systematic way with accuracy, variations

in space error or degree of constancy. This latter appears to be determined solely by the point around the arc of tilt occupied by the shape and the actual cues available to its perception. In the case of moving shapes (at this velocity) subjective variations in attitude seem incidental to the external objective situation.

TABLE III (b)

WIRE OUTLINE SHAPES MECHANICALLY ROTATED

80°	70°	60°	50°	40°	30°	20°	10°
1·0	1·42	1·66	1·84	2·24	2·33	2·87	2·9
1·0	1·84	1·9	3·33	3·2	3·0	2·74	2·65
1·0	2·24	2·83	4·0	3·09	2·24	2·45	1·73
0	1·41	2·12	3·32	4·12	4·06	2·34	2·12
0	2·55	2·24	2·45	3·0	3·09	4·0	2·35
1·0	2·12	2·24	1·87	2·92	3·16	3·87	3·0
1·0	1·41	1·73	2·83	2·12	2·34	2·23	1·58
1·0	1·22	1·58	3·08	1·73	1·58	2·0	1·41
1·0	1·22	2·0	1·87	2·23	2·73	2·45	2·12
·7	1·3	2·12	2·32	3·08	3·87	3·87	2·34
·77	1·55	2·04	2·69	2·77	2·83	2·88	2·22

σ of mean space errors for each subject. Pooled mean for group.

TABLE IV

MECHANICALLY ROTATED SHAPES, WIRE OUTLINE AND SOLID FORMS

	80°	70°	60°	50°	40°	30°	20°	10°
Binoc. vision—								
Wire outline ..	·75	·44	·27	·29	·25	·23	·2	·22
σ S.E.	1·0	1·5	2·12	2·23	2·2	1·88	1·87	2·04
Monoc. vision—								
Wire outline ..	·45	·2	·21	·23	·1	·15	·11	·12
σ S.E.	2·23	2·64	3·08	4·79	4·47	4·69	3·87	4·12
Binoc. vision—								
Solid shape ..	·6	·44	·25	·29	·23	·2	·19	·2
σ S.E.	1·0	1·8	2·15	2·87	2·23	2·0	2·12	1·87
Monoc. vision—								
Solid shape ..	·37	·2	·19	·24	·11	·14	·1	·09
σ S.E.	3·0	4·47	2·73	5·29	4·58	3·05	8·66	4·47

Comparison of binocular and monocular vision for two subjects. N = 256.
First line of data gives Constancy Values.
Second line of data gives Space Error Dispersions.

Most comments after the experiment were similar to those recorded above. Most subjects thought the experiment interesting, and perhaps strangely, "rather difficult to do well" (no one could be disabused entirely of the idea that some test of ability was involved), though no amount of discussion could make quite clear what "well" or "badly" related to in terms of objective performance.

Turning to the recorded data, in the case of Tables II(a) and III(a) each constancy recorded is the mean of eight actual trials of one subject, covering his responses for

both directions of tilt (waxing and waning). The calculations of dispersion have been made direct from the angular settings. Thus the working units are degrees of angle and the constancy tables are calculated from them. For the purpose of the experiment a complete set of Thouless constancies was calculated for all possible declinations of the circle with any one comparison shape. Hence constancy ratios could be read off direct.

It will be appreciated that dispersion shown in the Tables is wider than that obtained for any one direction, since it incorporates the space error. It might be thought that such a procedure is unjustified, but in practice it was found that dispersions for subsequent matches in a given direction were extremely small. In obtaining the eight matches, the first response in each direction was ignored.

TABLE V

WIRE OUTLINE AND SOLID SHAPES MATCHED BY HAND SETTING (STEP BY STEP)

	80°	70°	60°	50°	40°	30°	20°	10°
Wire outline	·25	·22	·17	·18	·11	·08	·06	—
σ S.E.	2·4	4·05	5·5	6·76	4·24	4·12	4·06	—
Solid shape.. ..	·29	·25	·19	·19	·1	·07	·05	—
σ S.E.	2·12	2·23	3·08	3·16	4·0	4·06	3·87	—

For 10 subjects, two trials in each direction. N = 320.
First line of data gives Constancy Values.
Second line of data gives Space Error Dispersions.

TABLE VI

CONSTANCY VALUES FOR WIRE OUTLINES AND SOLID SHAPES (POOLED MEANS)

	80°	70°	60°	50°	40°	30°	20°	10°
Wire outlines ..	·118	·154	·16	·186	·23	·22	·199	·197
Solid shapes ..	·113	·115	·128	·174	·184	·182	·187	·207

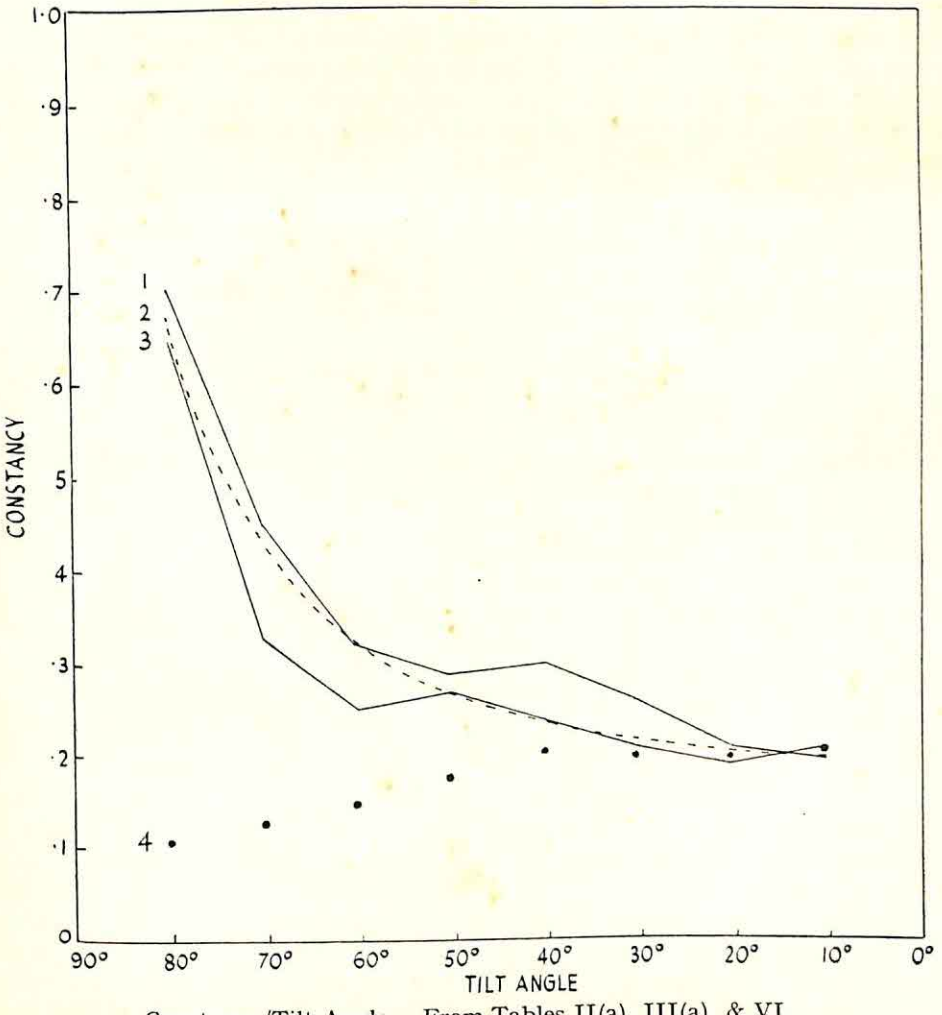
Treated by formula (P–S/R–S).Cos.S.

A characteristic of the results is the extreme similarity, subject for subject, between the solid shapes and the wire outlines. It would seem that under these conditions the wire outline is a superior object to perception, whilst the results for hand-setting (Table V) show the opposite.

However, the most interesting result is the marked and consistent nature of the curve of constancy through the arc of tilt. Table V for step by step setting gives similar results to those obtained by Thouless and the reverse of those obtained by Eissler, in showing a progressive decline in constancy toward the frontal-parallel plane. The curves from the experiment involving continuous motion all show an extremely high constancy toward the line of regard falling to a low point around 60°–50°, rising slightly thereafter and then declining once more as the frontal-parallel plane is approached. The phasic character of the contour persists in the records of every subject and is present under conditions of monocular vision (Table IV).

Graph I shows the records for the pooled mean through the arc of tilt placed, for comparison, adjacent to the curve of a simple *sine* function. This curve is obtained by taking an arbitrary constancy value and calculating plot points from the product of that value and the sine of the angle.

A surprising feature of the results was the stability and precision of recorded matches. Space error remained small and was almost always "overestimation" or "late response." Anticipating responses were rarely obtained (i.e. negative space error). It might be asked to what extent the speed of the moving shape influences the subject, so that his response is never a pure "space error" but a time



Constancy/Tilt Angle. From Tables II(a), III(a), & VI.

1. Table III(a).
2. Sine Function.
3. Table II(a).
4. Results treated by formula:— (P–S/R–S).COS.S.

error. Although it is believed by the present writer that so-called "space errors" in traditional comparison experiments cannot be taken as such at their face value, since the rate at which fresh estimates are called for can influence the score, there seems no reason to suppose that, at the velocity used here, any special temporal factor is in evidence more than in ordinary paired comparisons. Results from experiments already reported (Langdon, 1951), and others to be described later, appear to confirm this over a wide range of speeds.

XI

Discussion

It can be granted that the experiment has succeeded in one particular, if in no other, in that subjects retain a clear and unchanging notion of the "real object" throughout its vicissitudes. The continuing identity, the regular motion, together with certitude in belief as to what they were experiencing, together with the accuracy and consistency of their responses, appear convincing on this point. At the same time it is recognized that these phenomena may constitute a special class of experiences from which it may be illegitimate to generalize to those of the traditional type of experiment.

If the continuing identity of the shape is retained unchanged, what bearing do these results have upon Koffka's argument? Here the results are not quite so clear. It is obvious that the measure of constancy, as calculated from the Thouless formula, undergoes variations—though for reasons, it seems, other than those advanced by Koffka—but are such variations truly alterations in the character of the *percept* through the arc of tilt, or are they products of an over-simple method of treatment?

The basic formula developed by Thouless and Brunswik takes the form P–S/R–S where P is the percept or phenomenal match, S is the stimulus value and R the real object. Thouless replaced the numerical values by Log. values and Brunswik (1933) has shown that such notations may be used to make the formula independent of the absolute values in any situation. In the present case the arc of tilt involves a decrease in minor axis of the circle, seen projectively, by the *cosine* of the tilt-angle. Change of apparent shape will increase throughout the arc, angle for angle, according to this function. But replacement of the simple formula by such a function (i.e. Cos.P–Cos.S./Cos.R–Cos.S) does not alter matters, since this procedure affects both sides of the ratio equation and the non-linearity of the scale is not counteracted. As a rough approximation a non-linear angular scale can be achieved by plotting constancy values according to a sine function, viz. (P–S/R–S).Cos.S. Mean values for solids and wire outlines treated this way are given in Table VI and appear as plotted points in the graph.

The aim of such a procedure is to query the appropriateness of the simple constancy formula applied through the arc of tilt, and also to see whether the straight-line contour implied by the hypothesis of an invariant shape/tilt relation can be produced by some simple function.

Whilst the resultant contour may be approximated to a straight line, the mere fact of equal constancy values does not amount to very much. For in the untreated contour there may be seen repeated variations, clear rises and falls in the middle of the arc which cannot be ignored. Some of these variations are significant in the conventional sense, others are not. But this sense of significance has to be extended when small variations are ceaselessly repeated—as is found here—and there is no one function which can produce a uniform constancy with such data.

Apart from these considerations, it must be realized that the total set of perceptual conditions through the phases of the tilt arc are varying. Thus there is better "anchorage" for estimates of shape near the reference planes (line of regard, frontal-parallel). At the same time, these reference planes are non-equivalent since shapes orientated to line of regard may be sharply differentiated from one another and from the circle, whilst when orientated toward the frontal parallel, far less easily. And Stavrianos has pointed out in her study that there is poorer anchorage to the accepted reference planes at intermediate tilt-angles, and cites Hsia's work on whiteness

constancy as providing analogous confirmation of this. His suggestions are reminiscent of the argument developed by Eissler and reported above.

But in the context of present experiments this argument does not appear very convincing. If the increase in constancy is due, as Stavrianos, Eissler and Hsia suggest, to "over-compensation" by the subject attempting to counteract the loss of a strong plane of reference, why should there occur a great increase in constancy at the line of regard when such a reference plane is being approached? It might be objected that loss of the plane of reference must involve a fall in accuracy of judgments, and the fact that this does not take place is evidence that there is no such loss of "anchorage." But this cannot be accepted since Stavrianos has herself pointed out that "Accuracy of judgment of *tilt* was found to decrease significantly at the intermediate angles of inclination . . . . (but) the accuracy of *shape* perception was not found to vary systematically as a function of the angle at which the Standard was presented" (p. 92). Thus it would seem premature to entertain with any degree of confidence such speculations on the rôle of planes of reference, solely on the basis of evidence so far obtained.

The assumption of an invariant relation between shape and tilt carries the implication that the data may be fitted to a smooth curve of a simple function, and it is possible to find an expected theoretical value at any point along the contour, using the existing parameters. If this is done for the data of Table IIIa by the method of least squares, then for observations of the 40° range (where the rise in constancy takes place) the expected value is ·26 and the observed value is ·3.

It would be convenient to apply the χ^2 test to the data at this point and try to find the goodness of fit of such a calculated curve corresponding to this simple function. This is hardly possible to obtain from pooled data however, since there is no reason why a pooled mean of constancy values should tend to a normal, or any cumulative distribution. Each subject has his own constancy level (cf. Sheehan, 1938) and all that a group show in common are changes in relative values with changes of perceptual conditions. It is therefore necessary to apply a cruder test to the pooled data.

If it is assumed that the drawn point of the functional curve is approximately correct, it might be expected that the distribution of observed scores about its mean should indicate the likelihood of coincidence with the observed mean. An examination of raw scores, however, shows that of a population of 160 observations only 42 would lie on the further side of this theoretical mean, with the remaining 118 on its upper half.

Thus it would seem that although the variation from expected values is small, the recurrence of this variation in a large population gives it importance. A calculation from two sets of data for subjects 1 and 6 Table III(a) at 40° and the same subjects for Table II(a) at 50° (where a similar rise in the contour for solid forms occurs) gives values of "P" taken from Fisher's Table of χ^2 (1932) as ·2 and ·16 respectively which indicate only medium to poor fit. Subsequent experiments have, however, confirmed these tendencies to phasic shifts within the arc of tilt under extremely varied conditions. This has made the task of demonstrating statistically the precise degree of improbability of a simple functional curve seem superfluous, since the constant variations may be seen by inspection.

To summarize the results from these experiments the following points may be noted:—

1. Mechanically rotated shapes seen in continuous movement exhibit constancy of shape similarly to stationary forms.

2. Supplementary cues from surface texture and illumination do not appear to play an important rôle in maintaining constancy, whereas they do appear to do so in the case of stationary shapes.

3. The contour of constancy over the arc of tilt differs markedly from that obtained with stationary shapes.

4. This contour appears composite and irregular, and the probability of its corresponding to any simple function appears small.

5. Koffka's hypothesis of an invariant shape/tilt relation appears to derive little support from these experiments, although they are not absolutely conclusive.

6. It is possible that some non-linear function is involved in interpreting the results, but there is no reason to assume that one element determines the whole of the contour. Subjective factors may well enter as secondary determinants.

7. The situation created by mechanically rotated shapes is stable and clearly perceptible to the subject.

8. The subject's reactions appear to be independent of his conscious attitude and opinions: he is "drawn into" the situation, and makes estimates corresponding to the "synthetic attitude" described by Brunswik.

Further experiments will attempt to refine certain aspects of the situation which have so far remained rather vague, and to bring further variables under experimental control. This requires an amplification of the simple definition of shape given on page 90. This definition is inadequate in at least two respects; firstly, in that it ignores the role of the metric space in which objects exist, and secondly, it does not differentiate between different classes of forms.

Traditional investigation of shape constancy has contented itself with studying forms which occupy two dimensions and merely define a third by their own movement (i.e. by tilt), whilst the constancy of shape present in everyday perception is usually an outcome of interaction between numbers of solid three-dimensional objects within a spatial setting. Besides the integrated redistribution of boundaries characteristic of changing shapes, there is then, the question of the influence which may be exerted upon the perception of them by the visual and spatial properties of their surroundings. As regards this latter, the work of Witkin and Asch (1948) and of Ames and his associates (1946:1951) suggests the need for future studies of shape and form perception to take environmental properties into account, and it is with such questions that later studies are to be concerned.

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FIXATIONS, POSITION STEREOTYPES AND THEIR RELATION TO THE DEGREE AND PATTERN OF STRESS

BY

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We have tested Maier's hypothesis that the strength of behaviour stereotypes elicited in an insoluble problem situation is a function of stress. Forty-six white rats of Wistar stock were split into two groups, which were given an insoluble problem in a water discrimination unit under two widely different degrees of punishment. Those animals who developed position stereotypes were assigned to two subgroups within each group and were given a soluble discrimination problem under conditions of 50 and 100 per cent. punishment respectively. The strength of stereotype was measured in terms of the stereotype-breaking score.

It was found that animals trained under low punishment developed the more persistent stereotypes. Animals trained under 100 per cent. punishment developed the less persistent stereotypes and the longer spans between stereotype breaking scores and the learning criterion. The behaviour of four animals in the low punishment group was closely akin to what Maier calls "abnormal fixation"; the subgroups containing them were not unimodally distributed. Punishment differentiated response latencies which furnished evidence that the fixated animals mastered the soluble problem, in spite of not responding appropriately.

I

Introduction

Systematic investigations of stereotyped responses are of considerable theoretical importance in view of the *stress-frustration-fixation* theory recently formulated by N. R. F. Maier (e.g. 1949). In a previous study (Knöpfelmacher, 1952) we tested the deduction from Maier's theory that reward should decrease and not increase the probability of extinguishing a stereotype. Our results showed that the strength of stereotype, measured in terms of extinction scores, varies positively with the amount of reward, and are in complete accord with *goal-motivational learning* theory, but contradict the *stress-frustration-fixation* theory. It might be maintained, however, on the basis of the *stress-frustration-fixation* theory that the degree of punishment or stress to which our animals were subjected was not sufficient to touch off the frustration mechanism and that for this reason our experiment cannot be regarded as critical. Whether such an objection could be valid depends on the direction of covariation between punishment and stereotype strength. Maier claims that stereotype strength varies positively with the amount of punishment, once the frustration threshold has been reached, and that the probability of reaching the frustration threshold is also a positive function of punishment. Hence the frequency of behaviour which deviates from expectation based on *goal-motivational learning* theory is regarded as a function of stress. The designing of experiments to test this hypothesis is hampered because Maier does not give an adequate operational "definition" of "stress" in terms of the stimulus variable alone. Differences in stress are merely inferred from the differences in the distributions of the stereotype-extinction scores, and, since these very differences are attributed by Maier to variation of stress, the argument is circular, at least in so far as it is supported by experiment. To get over this difficulty, we shall regard stress as a function of punishment consequent upon a response sequence which an animal is forced to make. This definition is close to Maier's experimental design and does not commit us to any physiological

models of the stress syndrome. In an insoluble problem situation an animal is forced to respond and cannot avoid punishment by modifying its response pattern, as it can in a soluble problem. According to Maier it is mostly in such situations as the former that frustration is developed and behaviour becomes adaptation-neutral.

The probability that behaviour is instigated by frustration depends, according to Maier, also on the pattern of presentation, and not only on the degree, of punishment. If, for example, an animal were punished on every trial instead of every second trial, the chance that frustration-instigated behaviour would develop would be increased. Maier therefore predicts that, if two groups of animals were to develop behaviour stereotypes in an insoluble problem situation under the same conditions, and if one group were then presented with the soluble problem under conditions of 100 per cent. punishment, the other under conditions of 50 per cent. punishment, more animals would develop rigid behaviour stereotypes in the former group. His predition was confirmed experimentally (Maier and Klee, 1943), but unfortunately the way in which he estimates the statistical significance of his data is unconvincing. If the relationship between pattern of punishment and resistance to stereotype extinction is what Maier claims it to be, it would be incompatible with goal-motivational learning theory, which requires that habits break the easier the more frequently they are subjected to extinction by punishment.

The present experiment tests the hypothesis that the rigidity of behaviour developed in an insoluble problem situation increases with increased degree and concentration of punishment. By comparing the effects of high and low punishment, both concentrated and diffuse, it can be determined, whether Maier's prediction about the positive effect of stress on stereotype rigidity is valid, and whether under conditions of 100 per cent. punishment rigid stereotypes are more frequent and more pronounced.

The effect of punishment on the behaviour of animals in an insoluble problem situation is tested (i) by observing the relation between degree and patterning of punishment and the stereotype-extinction score, (ii) by observing systematically the trend of response latencies in all phases of the experiment, (iii) by scoring the strength of an animal's escape reactions, and (iv) by observing the number of trials between the point at which a stereotype was broken and the point at which the new habit was learned. These trials will be referred to as breaking-learning spans and will be designated throughout by the abbreviation "B-L spans." By "response latency" is to be understood the time taken to make a choice response. The method of measuring it is explained in Section II. Both B-L spans and the trends of response latencies are important as indices of the differentiation between learning and behaviour stereotypy. Maier claims that the persistence of fixated behaviour is not due to absence of learning a more appropriate response, but to a compulsion to which the organism is subject, irrespectively of its having or not having learned. If resistance to responding in a soluble problem situation were differentiated with respect to the positive and negative symbol, this would be shown by consistent differences in response latencies to the positive and negative symbol. If such differentiation did not precipitate the extinction of stereotype and the appropriate new response, it could be held with some plausibility that the animals "know" but cannot "perform." Maier's hypothesis that fixations are not due to absence of learning the soluble problem would then be supported. The length of B-L spans gives information as to whether an animal abandons position stereotypes by virtue of some other mechanism besides learning the soluble problem. Variations in length of B-L spans may show e.g. to what extent stereotyped behaviour in a soluble

problem situation masks the progress in learning. The series of trials which constitute the learning score consists firstly of trials to break the stereotype and, secondly, of the B-L span. By treating these as separate variables, it may be possible to find out how far speed of learning is determined by resistance to stereotype extinction, and how far by the mere relative difficulty of forming the appropriate responses to differential cues, under varying conditions of punishment.

II

Method

The procedure is outlined in Table I. The animals were treated identically only in phase 1. The independent variables—degree and pattern of punishment—were varied in phases 2 and 3. Only animals who failed to break their stereotyped responses in phases 3 and 4 respectively entered phases 4 and 5 respectively.

TABLE I

Research Design

	Phase				
Group	1	2	3	4	5
Ia	Preliminary training	Insoluble problem 8 scs. detention	Soluble problem 8 scs. detention 50% punishment	Soluble problem 80 scs. detention 50% punishment	Guidance
Ib	,,	,,	Soluble problem 8 scs. detention 100% punishment	Soluble problem 80 scs. detention 100% punishment	,,
IIa	,,	Insoluble problem 80 scs. detention	Soluble problem 80 scs. detention 50% punishment		
IIb	,,	,,	Soluble problem 80 scs. detention 100% punishment		

Apparatus

Figure 1 is a diagrammatic representation of the water-discrimination unit used through-out the experiment. It was designed and built by Jonckheere (1953). The present author rounded the corners and modified the escape door unit.

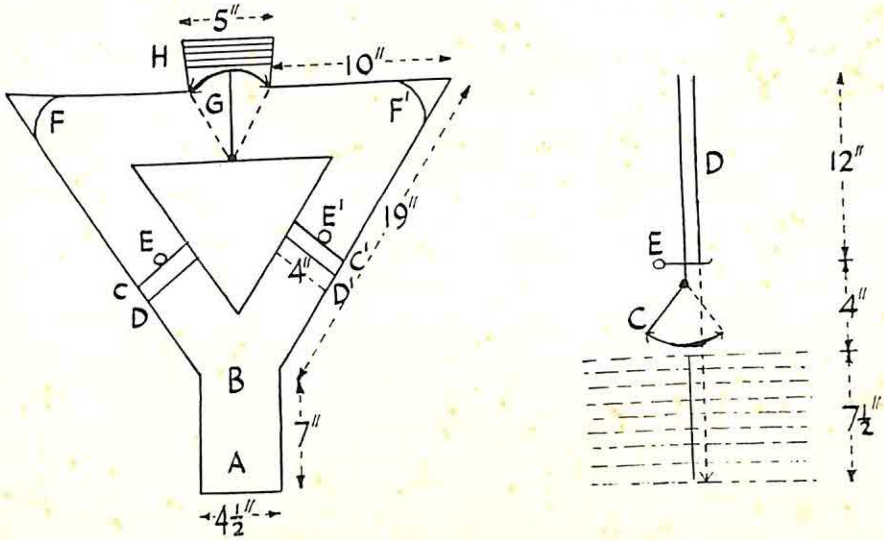
Subjects and techniques of grouping

Forty six male albino rats of Wistar stock were used. The animals were 3 months old at the beginning of the experiment. After phase 1 they were divided into two matched groups on the basis of tendency to form position habits and time required to make a response. The tendency to form position habits was assessed as the ratio of responses to the preferred side, over the total number of responses. The time to make a response was the time from placing the animal in the apparatus, to its choice of an escape door. This time interval will subsequently be referred to as response latency. The two groups formed were treated differentially in phase 2. Each group was then divided into subgroups a and b, matching on the basis of the number of trials required to reach the stereotype criterion. The animals were housed in a special series of cages in the animal colony room and were maintained on a diet of Thompson rat cubes and water.

Procedure

The water in the discrimination unit was kept at 18 ± 1° C. during all experimental periods, room temperature approximately 70° F. The animals were brought in small groups of 3 to 6 from the adjacent animal room into the experimental room and placed into cages at a convenient distance from the apparatus. From there they were taken out singly and placed at the starting point of the discrimination unit. As soon as an animal reached the top of the escape ladder it was put back into the cage in the experimental room, and the procedure was repeated with the next animal. In this manner rest pauses were introduced after each trial. This was important in practice as the research design required long periods of exposure in water which, with trials in close succession might have led to exhaustion and drowning. The number of animals taken into the experimental room was varied, so as to keep the rest intervals approximately constant irrespective of phase or group. After each trial the following data were recorded: Position of chosen escape door, punishment by detention or its absence, duration of response latency, and the part of the escape ladder on which the animal stopped. A daily session consisted of ten trials given to each animal throughout all phases of the experiment subsequent to preliminary training. At the end of a day's training the animals were dried and returned to their home cages.

FIG. 1.—Apparatus.



GROUND PLAN

SIDE ELEVATION OF A DOOR UNIT

A = starting point; B = choice point; D = slide door; C = escape door; E = electric bulb; F = detention compartment; G = hinged barrier by means of which F or F′ can be turned into a detention compartment; H = escape ladder. Letters with dash designate right-hand equivalents of what is designated by letters without dash.

Phase I. Preliminary training

All animals were given preliminary training for three days. On the first day the animals were trained to swim to an escape ladder on the far end of a straightway 48 in. long. Each animal received ten such trials in succession. On the second day the animals were introduced into the discrimination unit. Both escape doors led to the escape ladder without detention, and only one door at a time was illuminated in random order. Six trials were given. On the third day the same procedure was followed, except that the animals were forced to swim through each door an equal number of times and that the number of training trials was increased to ten. The side of the door through which the animals were forced was varied in random order. Forcing the animals had

to be effected in such a manner as not to obliterate all manifestations of spontaneous position preferences, since the latter were required as data for splitting the sample into groups. The animals were therefore allowed to approach a door freely and only when they were about to swim through it was their way blocked by means of the slide door, so that the animal had to reach the escape ladder through the other door.

Phase 2. Insoluble problem

Both escape doors were illuminated and an insoluble problem presented by furnishing no cues as to which of the doors led to the escape ladder without a period of detention. Groups Ia and Ib were detained for 8 seconds, and groups IIa and IIb for 80 seconds, on 50 per cent. of their trials. The side which led to the escape ladder without punishment by detention was varied in random order. The criterion of stereotype was 100 trials to one side with no more than 3 deviations. If an animal did not reach the criterion of stereotype in 200 trials, it was not included among the experimental groups. In order to standarize the measurement of response latency, Jonckheere's criterion of a completed response was adopted (1953). This defines a response as completed when an animal pushes its head through the escape door to the extent of having both ears in the detention compartment.

Phase 3. Soluble problem

As soon as an animal reached the criterion of stereotype, it was presented with a soluble problem. One door at a time was darkened and made positive by consistently leading to the escape ladder without detention. The illuminated door was made negative by association with detention. The position of the positive and negative doors was varied in random order for groups Ia and IIa; groups Ib and IIb were presented with the negative door on the side of the stereotype at every trial prior to breaking of the stereotype. The requirements for breaking a stereotype were two responses to the non-stereotype side in one day's session of which the second marked the breaking criterion. The learning criterion was the first of seven consecutive responses to a positive door in a day's session. As soon as groups Ib and IIb reached the breaking criterion, the negative symbol was varied from side to side as for groups Ia and IIa so as to make the B-L spans of all groups comparable. All groups received the same degree of punishment as in phase 2; i.e. they were punished by detention of the same duration as in phase 2. If an animal failed to reach the breaking criterion after 150 trials it was advanced to phase 4.

Phase 4. Soluble problem—increased punishment

Animals of groups Ia and Ib who did not reach the breaking criterion in phase 3 were submitted to increased punishment, in that the period of detention following the negative response was increased from 8 to 80 seconds. Thus they received equivalent treatment in this phase as groups IIa and IIb in phase 3. No animal of the latter two groups failed to reach either the breaking or the learning criterion in that phase.

Phase 5. Guidance

Those animals who failed to reach the breaking criterion in 150 trials under conditions of phase 4, were prevented from making the negative response and were manually guided through the positive door. When about to swim through the negative door, the animal's further progress was blocked by the slide door and it was pushed through the positive door with a wooden spoon. Guidance was discontinued when an animal made two responses to the door on the non-habitual side spontaneously; it was then allowed to learn the soluble problem by trial and error.

III

Results

The results are shown in Table II. Thirty-nine of the forty-six animals formed position stereotypes. Four animals broke the stereotypes only under guidance.

TABLE II

RESULTS OF EXPERIMENT

No. of Column

1	2	3	4	5	6	7	8	9	
Group	*Animal*	*Insol. pr. acquis. trs.*	*Trials to break* 8 *scs.*	80 *scs.*	*B-L spans* 8 *scs.*	80 *scs.*	*Learning* 8 *scs.*	80 *scs.*	*Guidance*
Ia	1	200	150—	43		0		43	
	2	110	150—	28		2		30	
	34	100	150—	150—		7			1
	3	100	150—	47		2		49	
	4	100	150—	57		3		60	
	5	100	150—	35		5		40	
	9	100	150—	150—		2			7
	6	120	150—	150—		6			11
	7	200	150—	76—		14		90	
Ib	8	170	150—	9		31		40	
	10	100	150—	10		40		50	
	11	100	150—	17		3		20	
	12	100	38/112–	(17)		(5)		(20)	
	14	100	59		4		63		
	15	100	150—	26		24		50	
	13	100	150—	150—		0			6/9
	16	100	57		133		190		
	17	150	150—	8		22		30	
IIa	18	110		37		3		40	
	19	120		40		2		42	
	20	170		26		27		53	
	21	100		26		4		30	
	23	100		27		13		40	
	24	120		53		0		53	
	25	100		39		11		50	
	26	100		38		13		51	
	27	100		25		5		30	
	28	100		58		2		60	
	29	100		78		2		80	
IIb	30	120		17		15		32	
	31	100		25		15		40	
	32	100		15		15		30	
	33	100		15		5		20	
	22	200		43		20		63	
	35	100		14		36		50	
	36	100		12		11		23	
	37	120		15		26		41	
	38	100		5		35		40	
	39	140		17		34		51	

150— means that the animal was advanced to the subsequent phase after having failed to attain the breaking criterion in 150 trials. Rat No. 12 broke the stereotype after 38 trials and formed a stereotype to the opposite side; figures in brackets refer to the latter. Figures in columns 8 and 9 show number of trials only in that phase within which breaking occurred. Column 10 shows number of guided trials to reach the breaking criterion. Rat No. 13 formed a stereotype to the non-fixated side after 6 guided trials. The new stereotype was broken after 9 guided trials.

Breaking

The breaking scores were markedly differentiated by both degree and patterning of punishment. To analyse these and all other results, non-parametric tests were used throughout, since normality of distribution could not be assumed. Two by

TABLE III

Tests of Significance between Breaking Scores

Phases	*Groups*	>	*Median*	<	p
3	Ia	9	68	0	<0·01
3	IIa	1		10	
3	Ib	9	31·5	0	<0·01
3	IIb	1		10	
3	Ia	9	150	0	>0·05(1)
3	Ib	6		3	
3	IIa	8	26	3	<0·05
3	IIb	1		9	
4	Ia	6	43	3	>0·05(1)
4	Ib	1		5	
f 4	Ia	5	28	1	<0·025
4	Ib	0		5	
4	Ia	7	41·5	2	<0·05(1)
3	IIa	3		8	>0·05
f 4	Ia	4	39	2	>0·05(1)
3	IIa	5		6	
4	Ib	3	15	3	>0·05(1)
3	IIb	4		6	

f = fixated rats excluded. The uneven distribution in the third Table is due to the fact that most animals did not attain the breaking criterion in 150 trials. The data are split in a manner favourable to the null hypothesis whenever the median corresponds to a tied or untied real score. Only p values followed by (1) are one tail estimates. The others are two tail estimates.

two tables comparing experimental groups and above or below median score were tested for significance by the Fisher-Yates exact test. These are summarized in Table III. The following conclusions seem to be justified: (i) Stereotypes formed under high unavoidable punishment are easier to extinguish than those formed under low punishment, (ii) their breaking is facilitated if the soluble problem is set under conditions of 100 per cent. punishment and (iii) most animals who fail to abandon the stereotypes under low punishment do so when punishment is increased.

The four animals who did not reach the breaking criterion when punishment was increased reached it by guidance. Their behaviour, in this and other respects closely analogous to Maier's fixated rats, will be discussed in another paper. The animals will be referred to as "fixated" in accordance with Maier's terminology. Inspection of columns 4 and 5 of Table II shows some evidence that the breaking scores of groups Ia and Ib are not unimodally distributed. There is no such evidence with respect to groups IIa and IIb.

Stereotype acquisition trials

Column 3 of Table II shows the number of stereotype acquisition trials. Festinger's non-parametric test (1946) was applied to the difference between results of group I(a and b) and group II(a and b) and to the differences between a and b within I and II. No significant deviations from homogeneity were found. Hence degree of punishment did not affect the speed with which stereotypes were formed. Tests between a and b confirmed that the groups were fairly matched on the number of stereotype acquisition trials.

TABLE IV

Tests of Significance between B-L Spans

Phases	*Groups*	>	*Median*	<	*p*
3	IIa	3	13	8	<0·05
3	IIb	8		2	
f 4	Ia	2	5	4	>0·05(1)
4	Ib	4		1	
f 4	Ia	2	3	4	>0·05(1)
3	IIa	6		5	
f 4	Ib	3	23	2	>0·05(1)
3	IIb	4		6	

f = fixated rats excluded.

B-L spans

There are not enough data to compare the B-L spans of groups Ia and Ib with those of groups IIa and IIb in phase 3. No animal in group Ia and only three in group Ib broke the stereotype in that phase. Of the latter three animals, one

developed a rigid position stereotype to the opposite side immediately on reaching the breaking criterion and can therefore not be included for comparison. The B-L spans of the two remaining animals in group Ib are 4 and 133, of which the latter is the longest B-L span observed in this experiment. Table IV summarizes the Fisher-Yates tests of homogeneity between comparable groups.

TABLE V

TESTS OF INTERACTION BETWEEN BREAKING SCORES, B-L SPANS AND THE PATTERN OF PUNISHMENT

Group	*Variables correlated*	*τ values*	*p*	*Partial τ values*
IIa & b	1, 2	− 0·65	0·001	
in	1, 3	+ 0·57	0·003	2; + 0·32
Phase 3	2, 3	− 0·75	0·00	1; − 0·60
Ia & b	1, 2	− 0·73	0·0078	
in	1, 3	+ 0·65	0·02	2; + 0·61
Phase 4	2, 3	− 0·35	0·15	1; + 0·24

The figures in the second column designate correlated variables thus: 1 = pattern of punishment, 2 = breaking scores, 3 = B-L spans. The figures in front of the partial τ values designate the extracted variables. All p values are 2 tail estimates with respect to the non-partial τ values.

The results indicate that presentation of the soluble problem under conditions of 100 per cent. punishment tends to lengthen the B-L spans. Since the pattern of punishment (i.e. 50 and 100 per cent. punishment in phases 3 and 4) differentiates significantly the breaking scores as well as the B-L spans it is necessary to correct the estimates of functional relationships for interaction. The three variables with respect to which the corrections have to be made—pattern of punishment, breaking scores and B-L spans—are not linearly related and one of them is dichotomous; hence only non-parametric tests of interaction are applicable. Kendall's method (1948) of extracting partial τ coefficients was found suitable particularly since it can be applied to dichotomous variables, as shown by Whitfield (1947). There is one drawback to this method, in that no way of testing the significance of partial τ coefficients has been devised yet. The τ coefficients are summarized in Table V.

The relationship between pattern of punishment and B-L spans is brought out by the two τ coefficients in rows 2 and 5 of Table V. Their numerical values are, however, decreased when the effect of breaking scores is extracted. Conversely, the negative correlation between breaking scores and B-L spans (row 3) is diminished when the effect of the pattern of punishment is corrected for. It can therefore be concluded, though the probability of these assertions cannot be estimated, that the longer B-L spans of groups b have to be *partly* attributed to the fact that concentrated punishment accelerates breaking; the negative correlation between breaking scores and B-L spans is in its turn partly due to the earlier occurrence of breaking under conditions of concentrated punishment.

Learning

The tests between learning scores are summarized in Table VI.

TABLE VI

TESTS OF SIGNIFICANCE BETWEEN LEARNING SCORES

Phases	*Groups*	>	*Median*	<	*p*
			70		
3	Ia	9		0	<0·01
3	IIa	1		10	
			57		
3	Ib	8		1	<0·01
3	IIb	1		9	
			200		
3 + 4	Ia	5		4	>0·05(1)
3 + 4	Ib	3		5	
			50		
4	Ia	5		4	>0·05(1)
4	Ib	3		3	
f			43		
4	Ia	3		3	>0·05(1)
4	Ib	2		3	
			41		
3	IIa	7		4	>0·05(1)
3	IIb	4		6	
			50·5		
4	Ia	5		4	>0·05(1)
3	IIa	5		6	
			40		
4	Ib	4		2	>0·05(1)
3	IIb	6		4	

3 + 4 means that the combined score of phase 3 and 4 is being compared.

It can be seen that high punishment in phase 2 and 3 accelerates learning of the soluble problem. There is no significant relationship between speed of learning and pattern of punishment. Yet while there is no significant difference between the learning scores of groups IIa and IIb, these groups are clearly not homogeneous with respect to their breaking scores and B-L spans. This illustrates how easy it is to mask important differences, if the variables contributing to the learning score are not studied separately, since similar group averages in terms of learning scores, may be due to quite different factors. As the learning score is a function of the

breaking score and the B-L span, the relative contributions of these two variables to it were tested, using Kendall's method of extracting partial τ coefficient as before.

In group IIb the τ value for B-L spans and learning scores is significant and higher than the τ value for breaking and learning scores. In group IIa there is barely any correlation between B-L spans and learning, while the positive correlation between breaking and learning scores is statistically significant. In groups Ia and Ib, the pattern of intercorrelations, though mostly not significant, points roughly in the same direction as in groups IIa and IIb in phase 3. The correlation between B-L spans and learning scores in group IIb is increased, when the effect of breaking scores is extracted. When the effect of B-L spans is extracted, the positive correlation between breaking scores and learning is also increased. The pattern of intercorrelations warrants the following tentative conclusions: Speed of learning is determined both by the speed with which stereotypes are broken and by the length of the B-L span. Among animals trained under conditions of 50 per cent. punishment, variations in the speed of learning are primarily determined by variations in persistence of stereotype. Among animals trained under conditions of 100 per cent. punishment, variations in the speed of learning are primarily determined by the length of training which the animal requires *after* its stereotype had been broken (i.e. the variations are mainly determined by the length of B-L spans). These results are in complete accord with goal-motivational learning theory. Animals

TABLE VII

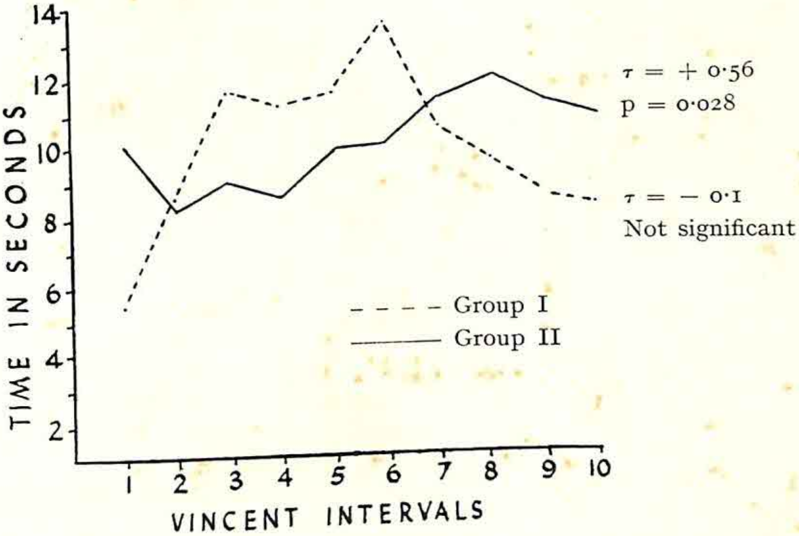
INTERACTION BETWEEN BREAKING SCORES, B-L SPANS AND LEARNING SCORES

Group	*Variables correlated*	*τ values*	*p*	*Partial τ values*
Ia in	2, 3	+ 0·48	0·272	4; 0·00
Phase 4	3, 4	+ 0·48	0·272	2; 0·00
	2, 4	+ 1·00	0·0028	3; +1·00
Ib in	2, 3	+ 0·20	0·91	4; +0·91
Phase 4	3, 4	+ 0·74	0·10	2; +0·77
	2, 4	+ 0·21	0·91	3; +0·06
IIa in	2, 3	− 0·55	0·026	4; −0·62
Phase 3	3, 4	− 0·11	0·689	2; +0·37
	2, 4	+ 0·64	0·009	3; +0·69
IIb in	2, 3	− 0·12	0·548	4; −0·50
Phase 3	3, 4	+ 0·69	0·008	2; +0·80
	2, 4	+ 0·32	0·234	3; +0·56

Correlated variables: 2 = breaking scores, 3 = B-L spans, 4 = learning scores. The τ and p values for groups Ia and Ib are based on very small numbers of animals, since rats who reached the breaking criterion in phase 3 or 5 had to be excluded from the test as their scores are not comparable with the rest of the respective groups.

in IIa and Ia are presented with the positive symbol on the habitual side and can therefore form the appropriate association even while they are still responding to the side of the stereotype. Animals in Ib and IIb on the other hand can form the appropriate association with the positive window only after they had ceased to swim to the side of the stereotype. The number of *negative* trials preceding the breaking criterion is roughly the same for groups a and b which shows that as far as persistence of stereotype is concerned a and b are not significantly differentiated (Table II). Hence variations of the learning score within Ib and IIb must be greatly influenced by what happens *after* the stereotype has been abandoned. Animals in Ia and IIa are rewarded for the choice of the positive symbol in between trials to the negative symbol and one could therefore expect that when avoidance behaviour with respect to the negative symbol is achieved approach behaviour to the positive symbol has meanwhile been built up.

FIG. 2.—(Group I and II in phase 2)

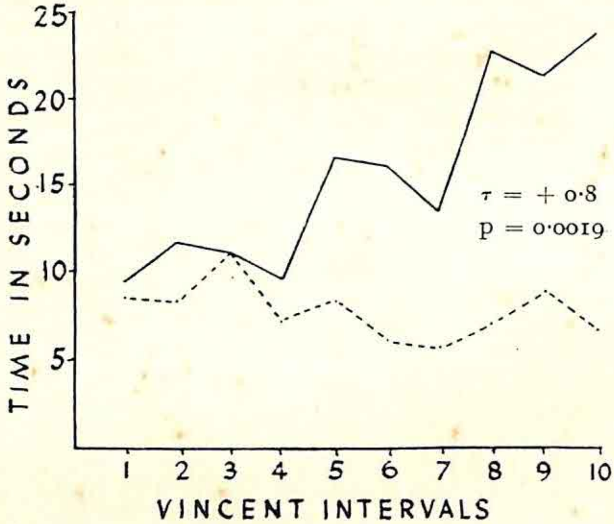


Response latency.

Figure 2 shows the change of response latencies in phase 2. The data were combined into *Vincent* curves within groups I and II separately. To test the trend of the curves the rank order of the latencies was correlated with the ordinal numbers designating the order of the *Vincent* intervals on the ordinate. The results, in terms of τ coefficients and their respective p values are shown in the figure. It can be seen that only the response latencies of Group II tend to increase consistently under conditions of an insoluble problem. The latencies of Group I rise steeply at first and drop suddenly after the sixth interval. Though no consistent trend was thus revealed in Group I throughout phase 2, the segments of the curve to the right and to the left of the sixth interval reveal significant sub-trends. The respective τ values are $-1{\cdot}00$ for the former and $+0{\cdot}86$ for the latter. Both figures are significant on a 0·016 level of confidence. The results seem to indicate that among animals trained under conditions of high punishment there is an increasing reluctance to respond. Among animals trained under conditions of light punishment, the initial increasing reluctance to respond is reversed into an increased readiness, as if this group became in some sense "reconciled" to the situation.

Figure 3 shows the response latencies of Group IIa in phase 3. Responses to the positive and to the negative door are plotted separately; all responses in phase 3 up to the breaking criterion are included. The gradual increase in discrepancy between the two curves indicates that the animals were able to discriminate between the positive and the negative window *before* reaching the breaking criterion.

FIG. 3.—(Groups IIa in phase 3)
——— Responses to − ve door
- - - - Responses to + ve door



The τ value in the figure has been obtained by correlating the ranked differences between the positive and negative response values with the order of intervals. Its significance confirms that the increase in discrepancy between response latencies to the positive and negative window is consistent.

Inspecting Figure 4 it can be seen that no significant differentiation of response latencies to the positive and negative doors takes place among group Ia in phase 3. As soon as punishment is stepped up the response latencies of this group begin to show consistent differentiation of the same kind as those of group IIa. Figure 5, which is a representation of group Ia in phase 4, shows this quite clearly. To see whether there is any difference between the trend of response latencies among the fixated animals and among those who broke the position stereotypes in phase 4, two separate pairs of *Vincent* curves were plotted for the two respective sets of animals. Inspection of Figure 6 reveals that fixated as well as non-fixated animals showed a signficant trend of increasingly differentiating response latencies to the two doors. In order to make a direct comparison between the changes of response latencies among fixated animals and among animals who broke in phase 4, their respective response latencies are plotted separately against the same time scale, throughout phase 3 and 4 in Figure 7. The absence of differentiation in phase 3 is characteristic for both sets of animals, as can be seen when we inspect the figure to the left of the vertical dividing line which marks the boundary between phase 3 and 4. As soon as punishment is increased, i.e. in phase 4, to the right of the vertical dividing line, differentiation of response latencies becomes apparent, equally sudden and pronounced for both sets of animals. Among the non-fixated animals the peak of differentiation precipitates breaking of the stereotype. The fixated animals on

the other hand continue to respond to the side of the stereotype even after maximal differentiation of response latencies, which moreover does not tend to diminish. This is of considerable importance, since it substantiates Maier's hypothesis that fixated animals "know" the solution of the soluble problem, but cannot perform.

Figure 8 represents the response latencies of group IIb in phase 3. These animals were not presented with the positive door on the side of the stereotype,

FIG. 4.—(Group Ia in phase 3)

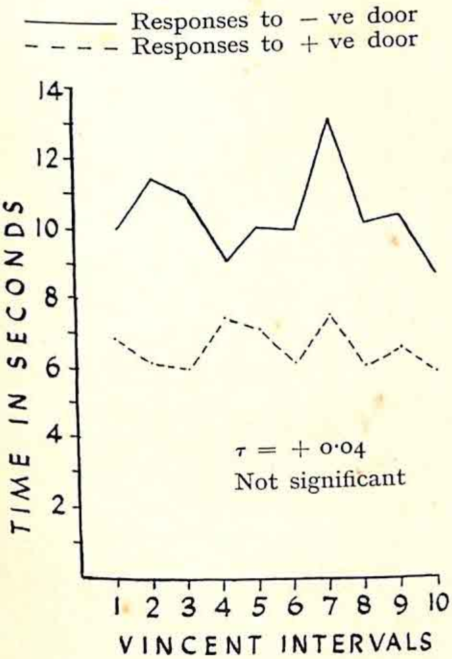
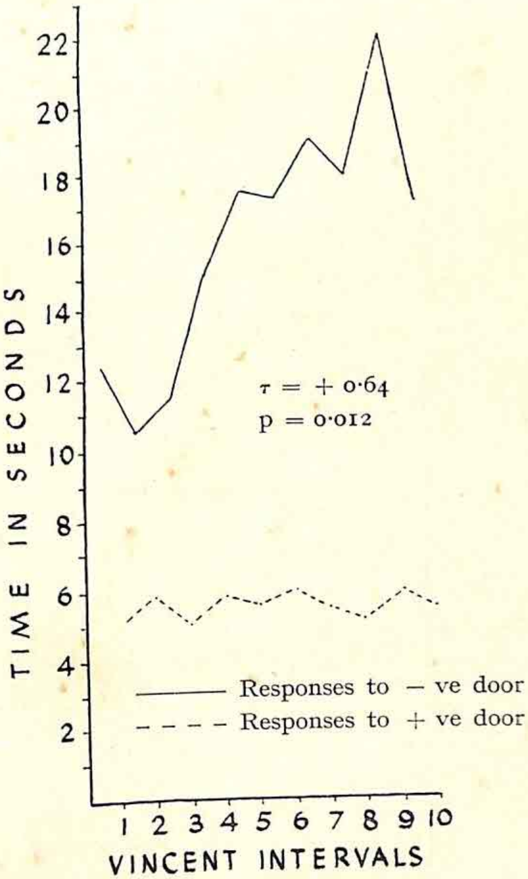


FIG. 5.—(Group Ia in phase 4)



and it is therefore impossible to infer anything about latent learning from differentiated response latencies. The increase in response latency to the positive door may however be interpreted as an indication of the animal's increasing reluctance to go to the habitual side and as a sign that the animal has learned to associate this response with punishment. There is, of course, nothing to indicate that the animal "knows" of the right door. Rat No. 22, whose response latencies were included in Figure 8, behaved quite differently as compared with the rest of the group. Its response latencies were much higher than those of any other animal in the group, reaching at one trial the record figure of 756 seconds; its breaking score was also well above the group average and the highest in the group. These and other qualitative aspects of its behaviour, to be described later, made it analogous to Maier's fixated rats. If this animal is excluded, the τ value of the curve in Figure 8 drops to $+ 0{\cdot}40$ and is not statistically significant. The response latencies of No. 22

are contrasted with the rest of group IIb in Figure 9. The two sets of latencies are plotted against the same time scale.

The response latencies of group Ib in phase 3 are shown in Figure 10. There is a barely significant consistent increase. Treating the latencies of animals who broke in phase 3 and those who had to be advanced to phase 4 separately, *Vincent* curves, whose values are 0·28 and 0·60, were respectively obtained. Of these only the latter is statistically significant (p = 0·008). We have therefore to conclude

FIG. 6.—(Group Ia non-fixated animals in phase 4)

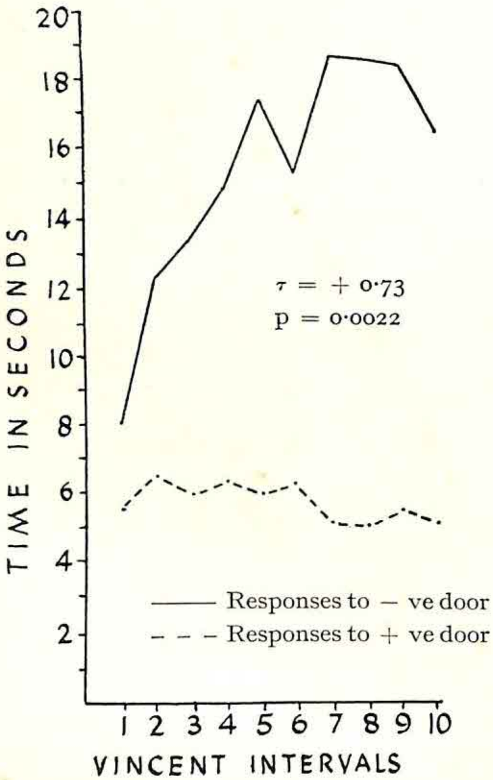
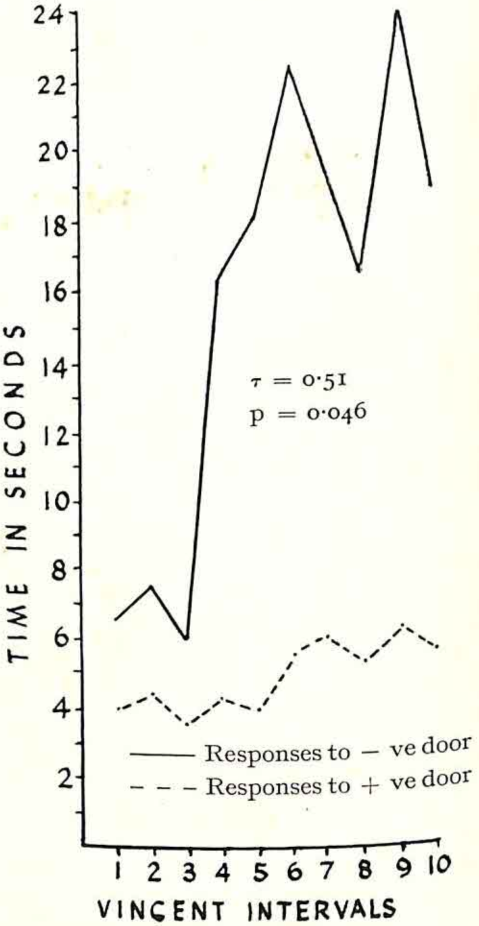


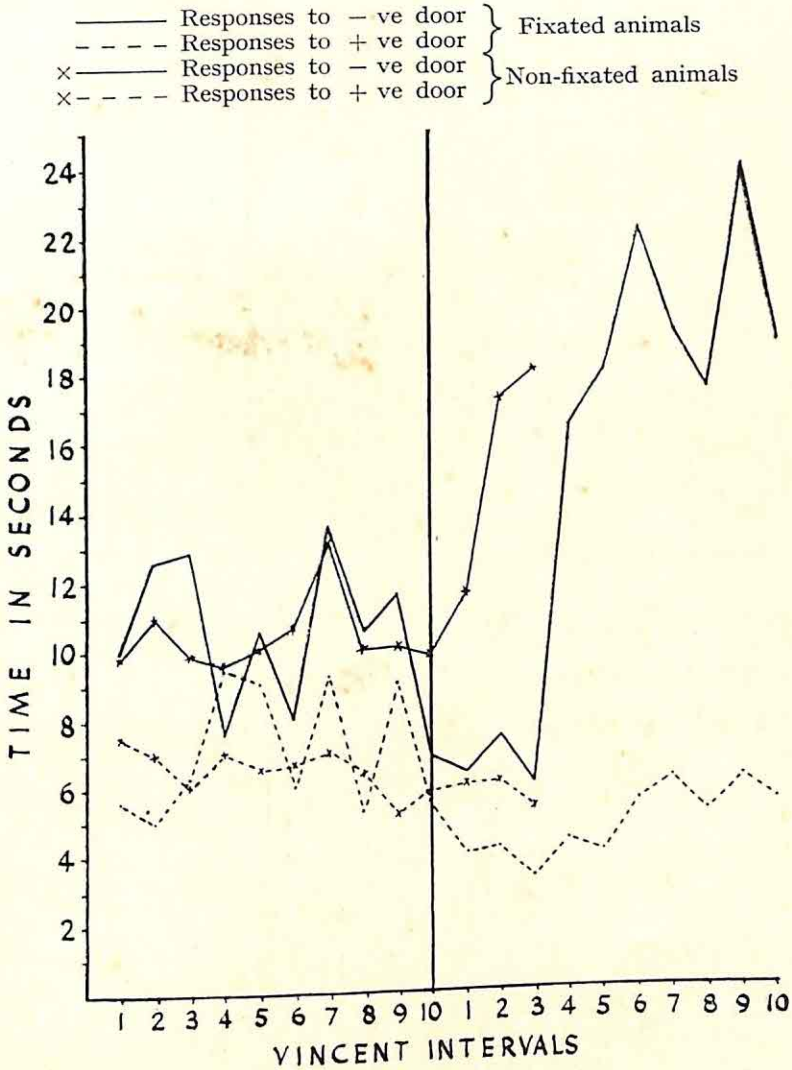
FIG. 6 (*contd.*).—(Group Ia fixated animals in phase 4)



that only animals who do not reach the breaking criterion in phase 3, show a consistent increase in response latencies. The trend of response latencies for the same group in phase 4 is shown in Figure 11, and is clearly significantly consistent. Rat No. 13 failed to reach the breaking criterion in phase 4 and showed most of the features commonly found among fixated rats. Its response latencies were much higher than the group average. When this animal is excluded, the τ value of the curve for response latencies of group Ib in phase 4 drops to 0·68 which is still significant (p=0·0046). Figure 12 shows the response latencies of group Ib in phases 3 and 4 against one time scale. Separate curves are drawn for animals who broke the stereotype in phase 3, for animals who broke it in phase 4, and for the

fixated animal No. 13. It can be seen that animals who broke their stereotypes in phase 3 responded with longer latencies than animals who did not reach the breaking criterion in that phase. This contrasts with the finding reported above, that the

FIG. 7.—(Group Ia throughout phases 3 and 4 against one time scale)



PHASE 3
(Low Punishment)

PHASE 4
(High Punishment)

latencies of the former animals did not consistently increase, whereas those of the latter animals did consistently increase. The small size of the samples makes it impossible to test the significance of the difference between the two sets of response latencies. Inspection of the curve for rat No. 13 reveals that the response latencies of this animal were far above anything shown by other animals of the group; the spectacular increase follows closely upon increase in punishment, which did not precipitate a comparable increase among animals who reached the breaking criterion.

FIG. 8.—(Group IIb, including rat No. 22 in phase 3)

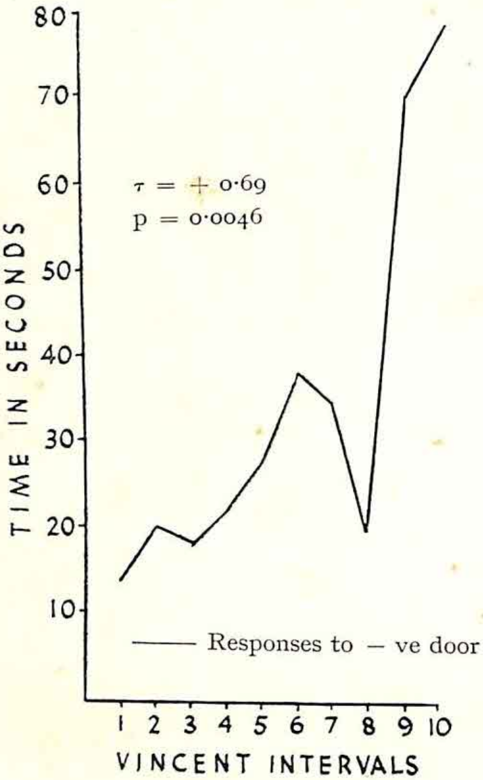


FIG. 9.—(Rat No. 22 contrasted with rest of Group IIb)

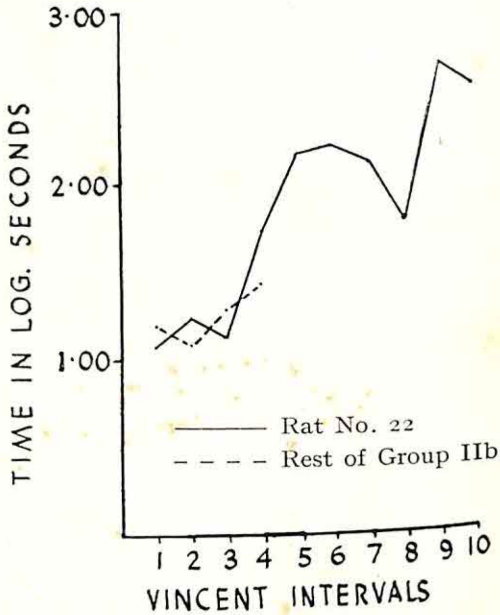


FIG. 10.—(Group Ib in phase 3)

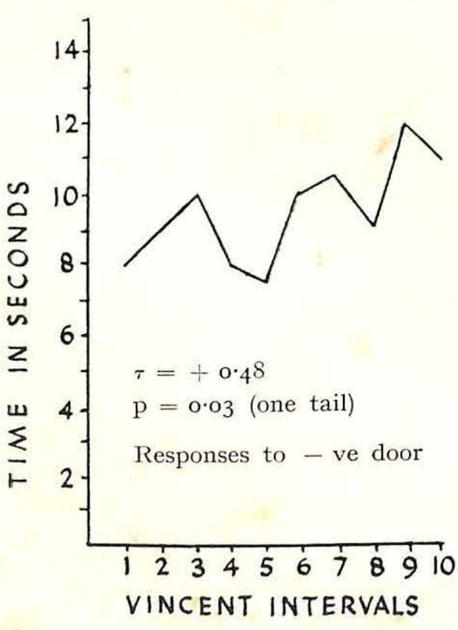


FIG. 11.—(Group I in phase 4, including No. 13)

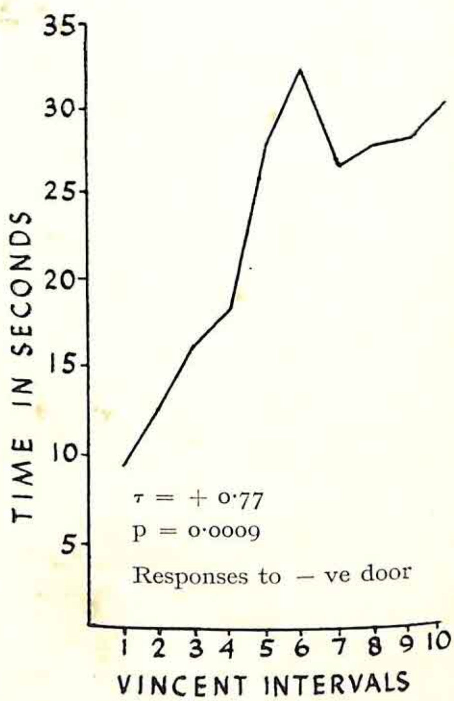
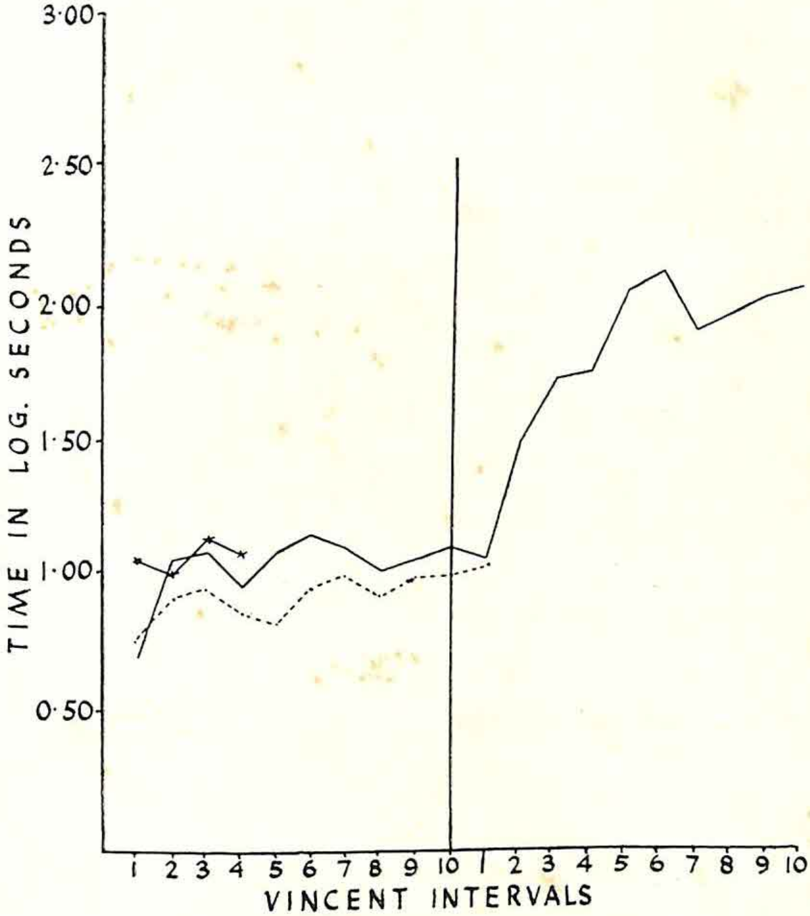


FIG. 12.—(Group Ib throughout phases 3 and 4)

×——— Animals who broke in phase 3
- - - - Animals who broke in phase 4 } Responses to – ve door
——— The fixated rat, No. 13



PHASE 3 (Low Punishment) PHASE 4 (High Punishment)

The analysis of response latencies in phase 3 and 4 warrants the following conclusions:

1. Animals develop differential response latencies to the positive and negative doors only under conditions of heavy punishment. When punishment is stepped up, differentiation of response latencies is precipitated.

2. Animals trained under conditions of 50 per cent. light punishment who do not break the position stereotype when punishment is increased, develop differential response latencies to the two doors, which remain differentiated despite failure to precipitate breaking of the stereotype. This can be interpreted as evidence that failure to reach the breaking criterion under conditions of increased punishment is not due to absence of discrimination learning.

3. Animals trained under conditions of both heavy and light 100 per cent. punishment develop a consistently positive increase in response latencies to the negative door.

4. One animal trained under conditions of light 100 per cent. punishment, who did not break the stereotype when punishment was increased, developed response latencies far above the average of its group. The animal with the highest breaking score in the group trained under conditions of 100 per cent. heavy punishment also developed response latencies far above the group average.

Strength of escape reaction

The animal's behaviour on the escape ladder presented an opportunity to obtain a rough estimate of the magnitude of its avoidance reactions. If longer periods of detention increase punishment, groups IIa and IIb ought to show a stronger tendency to escape from the detention compartment than groups Ia and Ib. The length of the animal's total escape route from the water was used as an index of the strength of its escape reaction; the assumption was that the stronger the animal's tendency to avoid the water, the higher up the ladder it will climb when an opportunity of escape is presented. The escape reaction was scored on a three point scale, for which the data were obtained by noting whether the animal would stop on the lower, middle or upper part of the escape ladder. The total escape reaction score was collected for each animal, by counting the number of times it stopped on any one of the three segments of the escape ladder, in phase 2 of the experiment. The segment on which it stopped most frequently is its preferred segment. To test the difference between the escape reactions of the two groups, the distribution of preferred segments was obtained for each group separately and the two distributions compared, applying the chi-square test. The results are shown in Table VIII.

TABLE VIII

THE DISTRIBUTION OF PREFERRED SEGMENTS ON THE ESCAPE LADDER

Part	*Group*	*Segment of escape ladder* 1	2	3	*Chi-square*	*p*
A	I	0	15	5		
	II	0	4	22	16·5	0·01
B	I	0	13	5		
	II	0	4	17	11·1	0·01

Part A shows the distribution of preferred segments for all animals used in the experiment; in B only those animals were included who formed position stereotypes to the required criterion. The p values are two tail estimates. The lower, middle and upper segments of the escape ladder are designated by figures 1, 2 and 3 respectively.

The results clearly indicate that animals detained for 80 seconds show stronger escape reactions than those detained only for 8 seconds. It can therefore be concluded that rats subjected to longer periods of detention in water are more strongly motivated to avoid it than animals detained for short periods only.

A qualitative description of some aspects of their behaviour will be given in a second paper, with a theoretical discussion of the whole experiment.

IV

Summary of Results

1. Both heavy and concentrated punishment facilitate breaking of position stereotypes.

2. Concentrated punishment increases B-L spans.

3. Learning scores of animals trained under conditions of 100 per cent. punishment vary significantly with B-L spans. Learning scores of animals trained under conditions of 50 per cent. punishment vary significantly with stereotype breaking scores.

4. The length to response latencies to the negative symbol is different from the corresponding length to the positive symbol only under conditions of heavy punishment.

5. Four animals in the group trained under conditions of light punishment did not break their position stereotypes when punishment was increased, though their response latencies were differentiated in a manner which strongly indicated that they did discriminate the positive from the negative symbol. The behaviour of these animals can be regarded as homologous with the behaviour of Maier's fixated rats.

Note: This is the first of two papers. The references will be given at the end of the second, which is to appear in the next issue of this Journal.

ON THE FIGURAL AFTER-EFFECT*

BY

F. H. GEORGE

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Two experiments were carried out on the figural after-effect. The first was intended to discover whether the figural after-effect did in fact take place with statistically significant universality. The method of scoring is relevant to the question of statistical significance, but at the best the figural after-effect appears to occur in only a slightly significant manner under the first conditions used. These involved the viewing of a single circle displaced to the left of a fixation point (the inspection-figure) and so placed that if it were superimposed on the second figure—which consisted of two squares on either side of the fixation-point—the circle completely encircled the left-hand square. The second figure is called the test-figure. The fixation point of the inspection-figure was fixated for 25 seconds with one eye, and then the test-figure was substituted and viewed with the opposite eye. A decision was then given by the observer as to the relative size of the squares. Eye-dominance did not appear to be connected with these after-effect processes.

In the second experiment, however, the statistical significance of the occurrence of the after-effects was undoubted. There were two changes in experimental conditions: the instructions to the observers were changed somewhat and a second circle was introduced into the inspection-figure. No difference of any kind was found to occur in the figural after-effect when the test-figure and inspection-figure were displaced horizontally and when they were displaced veitically, and there appeared to be a precisely opposite effect to that predicted when the test-figure and the inspection-figure were actually superimposed.

The most important finding was that, whereas in the first experiment the after-effect occurred with a frequency which was barely significant statistically, it occurred on every occasion under the different conditions of the second experiment.

I

Introduction

In many preliminary experiments difficulty was experienced in demonstrating the figural after-effect in anything approaching a consistent manner. The differences between individuals seemed sufficiently great in routine repeats of some of the Köhler-Wallach (1952) experiments to warrant a direct statistical test of the frequency of occurrence of the figural after-effect under specified conditions.

The figural after-effect can be obtained in the following manner. The observer fixates a single point in his static visual field, one part of which is occupied by a figure known as the inspection-figure, and then, after a suitable inspection period, of, say, at least 30 seconds, he fixates a point in a second static field, one part of which is occupied by a second figure called the test-figure. Changes in the second figure, particularly of size though also of displacement, colour, etc., constitute the effect.

Although Gibson was the first to describe the figural after-effect in detail, the work of Köhler and Wallach provide the bulk of the existing evidence. Köhler and Wallach claimed to demonstrate the existence of the figural after-effect for a large range of figures, and have also produced a theory, known as the "satiation" theory, which purports to account for their observations. But their work is limited by the fact that it is of a "clinical" character. No statistical treatments were

* I would like to acknowledge the help given me in the designing and building of the apparatus by P. Odescalchi (of the University of Bristol).

applied, and only small numbers of subjects were tested. Their evidence, indeed, would hardly be regarded as adequate in the realms of modern experimental design.

In the two experiments, to be described in some detail, an attempt was thus made to verify the statistical frequency of the figural after-effect. At the same time as these tests were carried out, other conditions were varied in order to discover whether they were relevant to the seeing of the effects. Thus, in the first experiment, besides checking on the frequency of the effect under Köhler-Wallach's conditions as against a control group, both left and right eyes were used in different subjects for viewing the test-figure. Eye-dominance also was assessed in every subject. In the second experiment horizontal and vertical figures were checked against each other and the results under both these conditions were compared with the result obtained when a card which contained both the test-figure and inspection-figures together was viewed.

The interest in these experiments was, in the first experiment, restricted to the question of whether eye-dominance was a relevant variable, whether the occurrence of the effect was statistically significant and whether the eye that was used to view the test-figure made any difference to the result. The fact also that the presentation was "monocular" proved that the effect was primarily central.

The second experiment was designed as a result of the first. The stimulus conditions were changed so that the possible effect of an enveloping circle (the circle of the inspection-figure was supposed to make the appropriate square of the test-figure relatively smaller) was supplemented by the opposite effect on the other square, thus perhaps doubling—certainly increasing—the effect, according to Köhler and Wallach. At the same time, the opportunity was taken to consider whether the effect was equally easily seen if the figures were displaced vertically from the fixation point.

The most interesting of all the comparisons was made possible by comparing also the figural after-effect under the standard conditions with the visual-illusory situation produced if the inspection-figure and test-figure were literally superimposed. The comparison here promised to throw some direct light on the underlying operation of the central visual system in these related situations.

II

Experiments

Experiment 1

The design of the first experiment was simple. Forty "naive" subjects were divided into two groups. Those in one group were shown first the test-figure alone, and then, after an interval of two minutes, the inspection-figure, for 45 seconds, followed by the test-figure again. Those of the other group were shown first the inspection-figure, then the test-figure, and then, after an interval of two minutes, the test-figure again. Thus two groups were formed, both having seen the test-figure, one immediately after the inspection-figure initially and the other independently of the inspection-figure initially.

Every subject was tested for eye-dominance (using a rectangular card with a centrally-placed rectangular hole and a simple stimulus object), and it was noted also whether he used his left or right eye for viewing the test-figure. This was important as the inspection- and test-figures were seen monocularly, each eye being used alternately, i.e., if the inspection-figure was presented to the left eye, the test-figure was presented to the right eye.

Experiment 2

The design of the second experiment was similar except for different inspection- and test-figures. This time the inspection-figure had circles both sides of the central fixation-point, one of which wholly contained (if the inspection-figure and test-figure are regarded

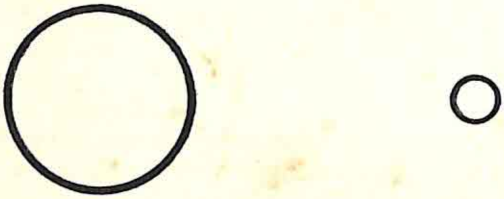
as superimposed) the appropriate square of the test-figure, while the other was wholly *contained by* the other square of the test-figure. Two sets of these cards were used; in one set the figures were being displaced horizontally from the fixation point, in the other vertically.

Thirty subjects were also shown the card in which the test-figure and the inspection-figure—both in the horizontal and vertical cases—*were actually superimposed.*

Apparatus

In one end of a plywood box of dimensions 3 ft. in length, 1 ft. 6 in. in width, 1 ft. 3 in. in height, were set two eye-pieces (small cylinders) which were fixed in a gap (approximately 3 in. × 8 in.) in the end sheet of plywood. The small cylinders were held in place by brackets; black cloth surrounded the eye-pieces, and wholly covered the remainder of the gap, being pinned by drawing pins to the plywood.

FIGURE I

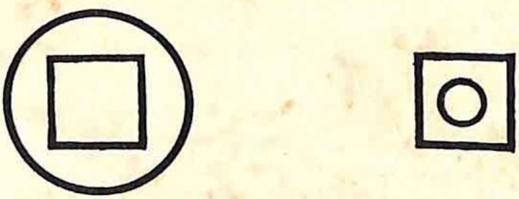


In the first experiment the small circle was omitted. In the second experiment the same figure was viewed both vertically and horizontally. When viewed vertically the large circle was on top.

FIGURE II

In the second experiment the test-figure was used both vertically and horizontally.

FIGURE III



Internal radius of large circle = 2·4 cm. Internal radius of small circle = 0·5 cm. Internal side of square = 2·3 cm. Distance between centres of squares and circles = 10 cm. Width of lines = 1·5 mm. Diameter of fixation point = 2 mm.

The fixation point was, in all cases, equidistant between the centres of the squares (or circles).

The whole of the inside, including brackets, flex, etc., was painted with black (flat undercoat) paint having a non-shiny surface. The external sheets of the box were screwed to batten-wood, which formed an inner framework. The sides were also clipped to each other externally to ensure the minimum of extraneous light.

Inside the box on an adjustable bracket was a screen, the top of which was 1 ft. $9\frac{1}{2}$ in. from the front of the box in the position used throughout the experiment and was tilted at a slight angle (3°–4° approximately) to the subject. The screen acted as a holder for the test-cards which were manipulated by the experimenter and pushed in and out

through a slot (1 ft. $\frac{1}{2}$ in. $\times$ $\frac{1}{3}$ in.) in the top of the box. This slot was covered by a black cloth during the experiment. The screen itself had attached to it a small piece of glass ($1\frac{1}{2}$ in. $\times$ 7 in.) which was blacked out except for a transparent circle at the top of radius $\frac{1}{4}$-in. Behind this circle was a ·5 amp. bulb (6·5 volts), connected through a knock-down switch and a transformer to the mains. This light acted as a fixation point since, when the test card was inserted, it was immediately behind a small hole (1/16 in. diameter approximately) drilled in each of the test cards.

The box was lit from each of the front corners by a standard gas-filled electric bulb (210 v. 100 w.). These bulbs were in a circuit which included a morse-key; the inside of the box was lit when, and only when, the key was depressed. In the first experiment, the test-figure used was of very simple design and was composed of the two white ("hollow") squares on a black background of *exactly* the same dimensions and placed symmetrically either side of the fixation-point. The inspection-figure was placed to the left of the fixation-point, and so placed that, if it was superimposed on the test-figure, it completely and symmetrically *encircled* the left-hand square of the test-figure.

In the second experiment, two test-figures were used, one being the same as in the first experiment, and the other being the same two squares displaced to the same extent from the fixation point, but in a vertical rather than a horizontal direction.

The two inspection-figures used in the second experiment differed, as did the test-figures, in that they were both horizontally and vertically displaced, and differed from the inspection-figure in the first experiment, only in that a small circle was added—to the right of the fixation-point in the horizontal case, and below in the vertical case.

The two figures for the simultaneous presentation of test-figure and inspection-figure merely required the superimposition of the horizontal, and the vertical test-figure with the appropriate inspection-figure.

The modifications in the second experiment were a result of the findings of the first experiment and should be carefully noted. The second (circumscribed) circle was added to the inspection-figure in the hope of increasing the figural after-effect; this small circle should, on the Köhler-Wallach theory, have the effect of enlarging the apparent size of the appropriate square of the test-figure, and then should work against the large (circumscribing) circle to decrease the apparent size of the other square. Thus, the second set of experiments should have a greater statistical significance than the first one with respect to the frequency of the figural after-effect.

Procedure

Each subject was tested and fell into one of two groups as previously described.

In each case the subject fixated the fixation-point for 15 seconds before the light inside the box was switched on for either inspection-figures or test-figures in both experiments.

Instructions

The instructions given to each subject were: "You will see a pin-point of light when you look inside this box and at the moment that is all you will see."

This is acknowledged by the subject, who is seated at the appropriate end of the apparatus.

"Now I am trying to find out how people see with the side of their eye. We know a great deal about the way people see with the centre of the eye but almost nothing about how they see with the side—since we know nothing about vision under such circumstances there is no question of you being 'right' or 'wrong' since we don't know what you *should* see.

"Now this is very important—to enable me to test your vision in the side of the eye you will see certain figures such as circles or squares which I will ask you about, as they are presented.

"It is essential that you continue to look at the pin-point throughout the experiment." Then when the test-figure is presented experimenter said: "Keep looking at the pin-point and say, if you are able, which square, if either, is the bigger?" In the second experiment, the phrase "as quickly as possible" was included although otherwise the wording was identical.

Subjects

All the subjects used were "psychologically naive" and knew only what was told them in the instructions about the experiment. They were drawn either from the R.A.F. or the civilian (non-academic) population of Bristol. All the subjects were male.

III

Results

Experiment 1

TABLE I

		Left square larger (*L*)	*Right square larger* (*R*)	*The same size* (*S*)
Group II	T-figure only first ..	12	4	4
	I-T sequence second ..	7	5	8
Group III	I-T sequence first ..	5	11	4
	T-figure only second ..	9	3	8

TABLE II

	Right eye dominant	*Left eye dominant*	
Right eye used for T-figure	8	4	Showing K.W. displacement
	2	2	Showing no displacement
	2	2	Showing opposite displacement
Left eye used for T-figure	4	4	Showing K.W. displacement
	3	1	Showing no displacement
	6	2	Showing opposite displacement

K.W. displacement means simply that the T-figure square encircled by the I-figure circle should look smaller if the I-figure is viewed before the T-figure. This is the change predicted by Köhler and Wallach (K.W.).

Total number of subjects = 40.

The results of experiment 1 are not entirely conclusive. A statistical examination of the basic figural after-effects are open to certain doubts. However, a direct comparison between the group seeing the test-figure only and the group seeing the test-figure after an inspection-figure (ignoring the second presentations in each case), gives a significant t-value. This is also true if consideration is given to the change in size occurring for either group (going through the order T-I-T or I-T-T). In every case, however, the value of t (see Table III) was not large, and this, in spite of the use of a scoring method that gave the maximum advantage to the figural after-effect. The method was to score + 1, − 1, 0, according to right square larger, left square larger, or same size square, in the first statistic, and in the second, + 1, − 1, 0, referred to displacement of the predicted kind (Köhler-Wallach), of the opposite kind, or no displacement. Now, if the division is made dichotomously into predicted change of size seen, or not, values of t so derived are *not* significant. Thus it seems probable that the results do not show wholly convincing evidence for the figural after-effect unless the "direction" of change of size is wholly ignored

and any change of size is regarded as exhibiting the necessary effect: this, however, has difficulties for the Köhler-Wallach neurological model—although it does give more significant t-values. A further interesting point is the fact that in the initial presentation of two equal squares, they were seen as equal by only 4 out of 20 subjects.

TABLE III

Statistic	*Value of* t	*Probability value*	*Conclusions*
1. Comparison of means between T-figure only first group and I·T sequence first group ..	2·62	$n = 38$ $t = 2{\cdot}01$	Significant
2. Significance of a single mean for Group I ..	2·33	$n = 19$ $t = 2{\cdot}09$	Significant
3. Significance of a single mean for Group II ..	2·33	$n = 19$ $t = 2{\cdot}09$	Significant
4. Compairson of means between two groups: dominant eye used on T-figure; non-dominant eye used on T-figure	2·35	$n = 38$ $t = 2{\cdot}01$	Not significant

In the above statistics + 1, − 1, 0 were used *in the first case* to represent right square larger, left square larger, square same size, and in the subsequent statistics + 1, − 1, 0 was used to represent change or displacement of the predicted kind, change of the opposite kind and no change at all.

A further analysis of the basis of the above scoring system shows that there is no statistically significant difference between any of the four groups of Table II which implies that there is no significant relationship between eye-dominance and the eye used to view the T-figure.

(I-T) figure means an inspection-figure (I) immediately followed by the test-figure (T). T-figure means a test-figure presented without a preparatory inspection-figure. This table is compiled without consideration of whether the left or right eye was used to observe the T-figure.

Total number of subjects = 40.

Experiment 2

TABLE IV

Group I ..	Left square larger (L) 30	Right square larger (R) 0	The same size (S) 0
Group II ..	Top square larger (T) 0	Bottom square larger (B) 30	The same size (S) 0
		Simultaneous	
Group III ..	Left square larger (L) 0	Right square larger 30	The same size (S) 0
Group IV ..	Top square larger (T) 30	Bottom square larger (B) 0	The same size (S) 0

Total number of subjects = 60.

The second experiment, however, seems to dispose of many of the difficulties raised in the first experiment, as in this case every subject saw the figural after-effect as predicted by Köhler and Wallach, in both the horizontal and the vertical cases,

and since the same test-figures were used in both experiments, for the horizontal displacement, it appears that the effect is sufficiently demonstrated.

Lastly, the results obtained when the test-figure and inspection-figure were presented together show unanimously opposite results to those seen in the second figural after-effect situation.

IV

Discussion

The above results seem to suggest that the figural after-effect is observable under suitable stimulus conditions, although the frequency of the phenomena is clearly changed considerably by relatively small changes in the conditions of viewing.

The striking difference in the results between experiment 1 and 2 is worthy of careful note. Only two conditions had been changed. The instructions to the subjects had been modified so as to ensure an immediate judgment, and the second small circle had been added to the inspection-figure. Indeed, it was observed in separate "pilot" experiments (not quoted here) that the small circle on its own seemed to have much more effect in producing the figural after-effect than did the large circle on its own. This observation, which seems important to the theory, deserves a further full-scale experiment, which might help to isolate more clearly the dominant variables.

However, apart from the above difficulties, one of the most interesting points to arise out of the first experiment was that some subjects in each group showed a change that was precisely *opposite* to the predicted figural after-effect. (There were four such cases among each group of twenty.) It seems doubtful whether an inability to register a "change" in one or two cases is significant, but when a displacement is registered and it is the opposite of that predicted, it merits some attention. Before discussing this, it is as well to consider the relation of eye-dominance to the figural after-effect. The results in this respect appear to be conclusive. There is no relation whatsoever between the "eye-dominance" of the subjects and their results in the figural after-effect tests, and this was corroborated in the second experiment.

Certain other facts of interest emerged in the comments of the subjects of experiment 1. When the subject gave an indefinite estimate of size such as a "left" judgment followed by another "left" judgment, he was asked to say whether the left figure was even larger or not so large, although still the larger of the two. The answers were varied and not sufficiently precise to be worthy of record. Secondly, size of the figures was the only estimate asked for since the question of brightness, fading, etc., seemed insufficiently definite; however, some eight or nine of the forty subjects voluntarily commented on the change of brightness which sometimes accompanied the change of size; others had usually noticed some change of brightness, if asked about it when the experiment was concluded; the same was less often true of "fading" effects and apparent displacement in three-dimensional space.

The second experiment raises some interesting problems. In the first place, the lack of significant difference between the horizontal and vertical displacement is of general interest not only to the figural after-effect, but also to general visual considerations. This cannot, of course, be taken as evidence that the figural after-effect is seen *equally well* in both the vertical and horizontal displacements, since, if the effect is well above threshold, this particular situation may fail to distinguish adequately between them, and since the results refer only to whether

the effect was seen or not, and does not consider *degrees* of the effect. Of even greater interest is the fact that the difference in size opposite to that of the Köhler-Wallach effect was seen in every case when inspection-figure and test-figure were presented together.

The above results—surprising in their unanimity—suggest that the temporal delay thought necessary for the figural after-effect to be seen is indeed necessary and appears to have a decisive effect on the visual system, although ambiguous figures, visual distortions, and the like, may be regarded with the figural after-effect as examples of some more general process. If this is so, then some unification is given to visual problems which they seem hitherto to have lacked.

There seems no reason to suppose that these results are not characteristic of the activity of the visual system. At the same time, it would be useful to have more quantitative data on the figural after-effect, as well as on the "visual illusions" that can be presented in two parts as inspection- and test-figures.

If the generality of these results are undoubted, the next step is to formulate a neurological model of the visual system (along the general lines of Osgood and Heyer) which explain "visual illusions," as well as the plateau spirals effects, apparent and real movement, and so forth. The importance of such a model needs no justification. In the meantime, further experimental data, referring to different conditions of viewing the figural after-effects are required.

The above results do not appear to be inconsistent with the general theory put forward by Osgood and Heyer (1952), although the writer thinks that this theory needs to be re-stated in a more specific form if the results reported here are to be explained.

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MISCELLANEA

AN OPTICAL MICRO-STIMULATOR FOR THE HUMAN RETINA

BY

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Purpose

As is well known, the application of micro-electrode techniques has provided new and suggestive data on the electrical activity of the retina. The data are so complex, however, that the functional significance of much of the activity is unknown.

In order to relate the electrical activity to photic stimuli it has been necessary to record from small areas of the retina and, in some cases, to stimulate small areas with light of carefully controlled intensity and wave-length. These special conditions of stimulation have raised problems of interpretation and have increased the importance of basic perceptual experiments using small spots of light. The study of perceptual phenomena associated with micro-stimulation might help to make it possible to interpret in detail the physiological records of retinal activity, and then to relate this activity to information used by the brain.

The instrument to be described is simple and versatile. It costs little and can be made by a mechanic without special experience of optical instruments. It is hoped that it may be adaptable enough for Class Demonstrations and sufficiently accurate at least for preliminary research of these problems.

Design

In order to reduce construction to a minimum the instrument is built round a conventional table spectroscope. This may be antique but should be of a heavy design and fitted with an accurate vernier scale. It is not necessary to damage the spectroscope, for the various attachments to the tubes may be mounted with clips made of spring brass and locked with tightening screws.

Normally table spectroscopes are so arranged that the collimator tube is fixed to the prism table while the telescope is free to swing, but for our purpose it is convenient to keep the emergent beam at a constant angle for all values of λ. This is achieved by simply screwing the collimator into the telescope ring mounting, and vice versa; the threads of the two tubes are usually identical. λ is now varied by swinging the collimator, to which a slow motion drive should be fitted. Once the instrument is calibrated, λ may be determined from the vernier scale, but, if a prism is used, the relation between the scale reading and the wave-length will not be simple, because a prism does not give a "normal spectrum." The prism should therefore be removed and a small replica grating substituted. λ is now determined by $\lambda = d \sin \Theta$, where d is the grating space (which for Rowland's machine is $1{\cdot}693 \times 10^{-4}$ cm.)

A lamp house is mounted on the end of the collimator tube; a suitable light source is a 6-volt car headlamp. Intensity must not be varied by varying the voltage, for this would change the spectral luminosity curve of the source, but by a neutral wedge filter which may be mounted on a shaft running along the collimator tube; the shaft is then rotated by the subject or the experimenter. The angle of rotation of this shaft is linearly related to intensity and may be measured on a scale attached to the drive end or, better, by means of a cyclometer counter geared to the shaft to provide a sensitive indicator of the angle. (A counter is more easily read at low levels of illumination; this is most important since readings must be taken quickly and fatigue avoided.)

We can now produce a beam of light of variable and controlled wavelength and intensity. We must use this to produce small fields suitable for retinal stimulation. Extremely small fields are usually produced with a microscope used backwards, a comparatively large field being reduced twenty to one hundred times (Hartridge, 1950). The present instrument avoids the use of reducing lenses, which are expensive and liable to systematic chromatic effects.

The small fields are obtained by reflecting the light from small steel balls. Ball bearings are used; they are very accurately figured and are made to a tolerance of 0·0001 inch diameter. Stainless steel ball bearings (down to $\frac{1}{8}$ inch diameter) may be obtained from the makers of model steam engines, in which they are used as steam-release valves. The size of the field is determined by the diameter of the ball and the viewing distance.

The angular separation between fields may be controlled by employing a ball bearing for each field and adjusting the distance between them with feeler gauges. For very small angles one ball may be used, this being lit by two beams and the angle between the beams being determined. Continuous adjustment of angular separation is possible by rotating the mounting holding the ball bearings with respect to the subject's viewing tube.

It is possible to keep the wavelength of each field the same, both being varied together with the collimator adjustment, or to keep a constant difference in λ between the fields as the spectrum is swept by the collimator. d λ is dependent on the setting of the surface-silvered mirrors m1 and m2. In order to vary d λ continuously, one of the plane mirrors may be rotated in a controlled manner, or the ball-bearing mounting may be rotated with respect to the plane mirrors.

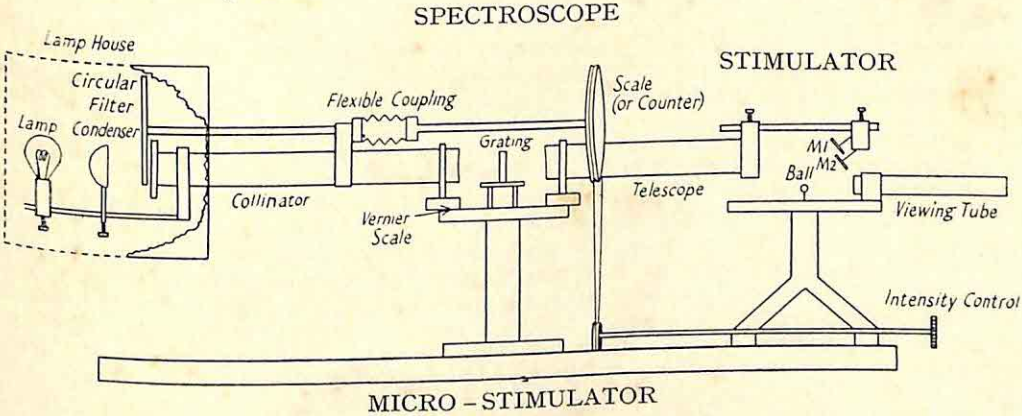
If the instrument is used to study interaction effects it is useful to be able to cut out each field. This may be done quite simply by mounting small vanes on relays, which may be electrically operated with keys by subject or experimenter.

Calibration

It is only necessary to calibrate on one spectral line since the collimator scale accurately follows a simple law. A sodium lamp, such as is used in street lighting, is used. The method is first to set the collimator vernier scale to the calculated value for the calibration line used (where d is $1 \cdot 693 \times 10^{-4}$, and λ is 5889, the scale should be set to 20° 28′. See: Jenkins and White, 1950). The plane mirror m1 should then be rotated until the orange sodium line falls onto the ball-bearing serving as spherical mirror and is seen with maximum intensity at the viewing tube. Any desired d λ between the two fields is now determined by setting the collimator scale to give the required difference and adjusting m2 in order to allow the line to fall on its spherical mirror; in this way the second field with the required difference in wavelength from the first is provided. It is thus possible to study interaction effects between small fields where these are of different colour. It is hoped that this technique might provide some information on the physiological basis of colour vision.

The stimulus variables which may be controlled are:—

1. One, two or perhaps more stimulus spots, or fields, are provided (*a*) simultaneously, (*b*) in turn.
2. The intensity of the fields is controlled, and at least relative measures up to 5 per cent. accuracy are possible. (Independent control of each field intensity is not described, but might be attained with a further calibrated filter interposed after m1 or m2. Shutters to produce flicker may also be incorporated.)
3. The difference in wavelength between the fields (*a*) may be continuously varied, or (*b*) may be held constant at any value while the visual spectrum is swept by the collimator.
4. Angular separation between fields may be set to any given small value, or continuously varied.



MICRO-STIMULATOR

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BOOK REVIEWS

GELDARD, F.A. *The Human Senses.* New York: John Wiley, and London: Chapman and Hall. 1953. Pp. x + 365. 40s. net.

The presentation of the senses to readers whose interest is predominantly psychological is a task bristling with difficulties and uncertainties, as anyone who ever seriously thought of attempting it knows very well. To what extent ought technicalities, physical and physiological, to be allowed to obtrude? How far, on the other hand, can they be glossed over without losing what is vital? In what way can sensory investigation best be shown to exemplify principles of method and approach capable of extension to other psychological fields less obviously amenable to scientific attack? Is it possible to display sensory mechanisms, not as distinct devices for encoding information from the environment, but as functionally related systems subserving adaptive behaviour? Is, perhaps, a comparative treatment too seldom adopted? Ought greater efforts to be made to bring the traditional chasm between the "sensory" and the "perceptual"? All these questions may reasonably be asked. But there will probably be agreement on two points at least. The first is that no single author, and certainly no single volume fit for human consumption, could be expected to compass the senses in an all-embracing fashion. The second is that the average student of psychology is apt to find little interest in the senses treated *seriatim* according to the traditional formula. His instinct is perhaps sound.

Dr. Geldard's book, except in one respect, will hardly lay him open to criticism on the score of unrestrained radicalism of design. Here is to be found, for the most part admirably expounded with a convenient amount of detail, most of the material which forms the special senses part of contemporary final-honours teaching in psychology. Fashionable topics in vision and hearing are neatly collected and set in a framework of classical controversy. The customary diagrams and figures illumine the text. Within the space of a hundred and fifty pages Dr. Geldard provides, for vision and hearing, as well-balanced and effectively pre-digested a sensory course of the psychological banquet as ever left a student's dreams untroubled.

It would be surprising if every reader's choice of material coincided precisely with one author's. The present reviewer would for instance have been glad to see a fuller account of Granit's micro-electrode work than can be given in four lines: some mention of Hartridge's micro-stimulation technique and polychromatic theory, however debatable they may be: at least a passing reference to the Stiles-Crawford effects, whose importance is perhaps comparable with that of the Bezold-Brücke phenomenon, which does secure some attention. The chapters on audition consist of a most competent summary of Stevens and Davis' *Hearing*, and to that extent disarm criticism.

The author's one departure from tradition consists, however, in devoting almost exactly half his book to senses other than vision and hearing. This has both advantages and disadvantages. It is true that a number of significant topics in the cutaneous, vestibular and kinaesthetic fields have often gained less attention than they merit in special-sense courses. But here, for instance, there is a clear account of the controversies stirred up by Head and Rivers, traced through to their fruitful outcome in recent work such as that of Woollard, Wedell and others. These chapters, however, contain other material perhaps less worthy of the space it occupies. They present a rather inflated appearance by comparison with the austerer standards of those concerned with vision and hearing. On the whole we must be grateful to Dr. Geldard for treating the "lower" senses at greater length than is customary, even though some may feel that his evident personal interest has led him into over-generosity in this respect.

The treatment of the vestibular mechanisms throws into relief a number of the difficulties mentioned at the outset of this review. Without appeal to a more general functional approach, without benefit of any adequate discussion of the central nervous mechanisms of posture, or of the movements of the eyes, accounts of the "vestibular sense" are apt to present a kind of *reduction ad absurdum* of the classical treatment of the senses. Dr. Geldard has made a heroic effort to overcome the difficulties by sketching, in a somewhat *ad hoc* fashion, something of the background required. The resulting chapter does not, however, leave a very clear impression of a subject in some respects absolutely central to the biological study of the senses.

This book will probably be read by many. Most of these it is certainly capable of instructing, and few will be misled. Fewer still are likely to be inspired. But for reasons already mentioned this can hardly be blamed on Professor Geldard.

R. C. O.

A Further Study of Visual Perception. By M. D. Vernon. Cambridge University Press, 1952. Pp. 289. 35s.

The study of perception has undergone very considerable developments during the past fifteen years. In 1937, when Miss Vernon's Visual Perception was published, the Gestalt psychologists dominated the field. Accordingly in that book full weight was given to their experimental contributions. But since that time much further experimental work has been done, the clear-cut Gestalt conclusions have been increasingly undermined, and the scope of experiment has itself been widened. A re-assessment of the older experimental work was needed, together with a survey of the new. We may well be grateful that Miss Vernon, in a largely new work, has accomplished this difficult but rewarding task.

In her earlier book Miss Vernon held that the time was not yet ripe for extensive hypotheses, and treated Gestalt theory with considerable reserve. Her caution is now justified. Most would agree that Gestalt principles appear today as a "one-sided exaggeration of certain features by no means the most important in perceiving as we ordinarily experience it." She also believes however, and this marks a new feature of the present book, that it is now desirable to state, "at least in very general terms, certain propositions as to the fundamental nature of the perceptual processes." Here she puts her finger on what is perhaps the essential defect of the Gestalt approach, that it fails to do justice to the constructive activity of the perceiver. Her propositions may be briefly summarized as follows. First, we must "emphasize what appears to be the most fundamental quality of the perceived field—its extraordinary unlikeness, so commonly overlooked in everyday life, to the stimulus field." The sensory impulses are only a part of the raw material of perception, and, as Bartlett demonstrated, there is much original construction. Second, it seems that "the individual constructs his perceived world as far as possible in accordance with the maintenance of the maximum of stability, endurance, and consistency," but at the same time maintains "an extreme sensitivity and the power of immediate response to all significant variation in the external world." Third, due recognition must be given to the importance of individual peculiarities which are inherent in these constructive processes. These propositions are discussed in connection with the development of concepts and schemata. Here Piaget is used effectively and it is suggested that the most hopeful approach to perception in the future may be "through the study of these schemata as they actually develop in the young child." As Miss Vernon says, "these propositions are perhaps too general to form a basis for predictive hypotheses leading to possible experimental investigation, and are in that respect less valuable than were the theories and hypotheses of the Gestalt psychologists. Nevertheless, these propositions have the advantage of being more inclusive and therefore less likely to produce a biased polemic such as disfigured so much Gestalt literature."

Her propositions also enabled the author to give a satisfying shape to the survey of experimental work which follows. Roughly speaking, the role of original construction becomes gradually more evident in succeeding topics. She begins by outlining "the fundamental stages in the process of perceiving" and related matters. The view that the transition from vague awareness, through knowledge of a generic object, to knowledge of a specific object, can be regarded as fundamental has sometimes been criticised on the ground that tachistoscopic experiments are artificial and misleading. Miss Vernon defends her standpoint convincingly however, and the evidence from cases of visual agnosia seems to provide a valuable alternative method of verification. Her use of brain injury material in general is one of the minor but useful new features of the book. After a chapter on the Determination of Form, which makes it clear that Gestalt principles are frequently no more than tendencies, fluctuating rather than persistent, we turn to Spatial Perception. The author picks her way through this tortuous topic with all the skill we should expect, and the addition of much recent American and Swiss research make this the outstanding section of the book. The treatment of the Constancies gives a much needed emphasis to individual differences and the significance of attitude. The section on "colour and brightness constancy" however leaves this problem as puzzling as ever. Perhaps some light might be thrown on it if more emphasis were placed on the distinction between whiteness and brightness made, as the author points out, by Thouless and Koffka. It seems doubtful whether there is anything which should properly be called brightness constancy. After describing the Functions of the Framework and the Perception of Movement, where the usefulness of her general principles is effectively shown, Miss Vernon gives a valuable summary of Michotte's work, which does so much to demonstrate the richness of the phenomenology of perception. She concludes with

the Influence of Internal and Individual Factors, discussing attention, set, motivation and types. The importance of original construction becomes abundantly plain here, and it is a great advantage to have the material brought together. The author's remarks on the influence of attitude and instructions in psychological experiments deserve particularly close attention.

The reviewer was left with two reflections. First, the term schema has always been vague and remains vague here. Perhaps we think of schemata too much in terms of their effects. It may be that if we are to achieve a more definite conception of their nature we must begin by studying the incoming stimuli and ask how the schemata must deal with these if they are to produce their known effects. Miss Vernon begins by saying that the most fundamental quality of the perceived field is its extraordinary unlikeness to the stimulus field. But this is nowhere taken up and examined. That is to say, the author never asks exactly what the schemata have to handle and exactly what the task of construction is.

The second reflection will be shared by all Miss Vernon's readers. The author's skill in sifting and collating the experimental work in this vast and unsettled field, and the balanced and penetrating judgment which she brings to bear upon it, constitute a contribution to psychology of the greatest value. All who are interested in visual perception will find the Further Study indispensable.

R. B. J.

Physiology of Vision. By various authors. *British Medical Bulletin*, Vol. 9, No. 1. London: Medical Department, British Council. 1953. 15s.

This number of the *British Medical Bulletin* is devoted to eleven papers on the physiology of vision. It is edited by W. S. Stiles and introduced by Sir John Parsons. There is also an excellent bibliography of British books and periodicals compiled by Katherine Tansley.

Among the papers are useful summaries of work available in full elsewhere—notably W. D. Wright's "Defective Colour Vision"—and accounts of new work, much of it of fundamental interest. The organisation of the retinal elements is discussed by L. C. Thomson in "Stimulation of the Retina with Light Fields of Small Size," by M. H. Pirenne in "The Absolute Sensitivity of the Eye and the Variation of Visual Acuity with Intensity," and by W. A. H. Rushton in "Electric Records from the Vertebrate Optic Nerve."

Both Rushton and Stiles—the latter in "Visual Properties Studied by Subjective Measurements on the Colour Adapted Eye"—are particularly concerned to relate the physiological findings to perception. Rushton, after discussing the classical microelectrode work of Granit, Hartline, Adrian and Mathews, asks: What is the significance of the optic nerve record? He points out that, unlike the case of the touch receptors, its response is too complex to be directly related to stimuli. This complexity seems to be in part due to retinal censoring of the available information; this censoring is found especially in such retinas as the frog's which only respond to restricted types of stimuli necessary to the creature's survival. The human *fovea* seems to be quite different from the frog retina; here, as in the very primitive eyes such as *Limulus*, the information is fed uncensored to the brain.

R. L. G.

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ADVANCE INFORMATION IN SENSORI-MOTOR SKILLS

BY

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Two experiments were carried out to demonstrate the effects of advance information. In both cases greater smoothness of performance was found to be possible when advance information was available. This effect was considered more important than the reduction in response time. Both effects were reflected in a reduction of the stopping times between successive responses.

I

Introduction

The experiments to be described in this paper formed part of a larger investigation an account of which may be found in Bartlett (1951). A fuller description of the experiments themselves is contained in a dissertation by the author (Leonard, 1952).

The importance of anticipation in sensori-motor skills has always been recognized. As a result of some of the problems posed during the second world war the rôle of anticipation became to be more closely studied. In its most elementary form—as when "reading ahead"—Bartlett has called it receptor-effector anticipation to distinguish it from that kind of anticipation which depends more exclusively upon past experience. It is appreciated that this term is open to criticism and it should be taken as no more than a distinguishing label.

Leaving out the more complex reading experiments there are only two sets of papers reporting investigations of this phenomenon in sequential tasks, and they are well separated in time. Cattell studied the matter as part of his experiments on the recognition and identification of letters, and Poulton (1950) carried out some of the earlier work in Cambridge. Cattell's paper is of more than historical interest for he established not only that a maximum of 4 to 5 letters exposed at any one time had the best effect, but also that under those conditions letters were read out at a rate of 4 to 5 a second.

Poulton (in reports to the Medical Research Council) has described some of the difficulties experienced by him when working with receptor-effector anticipation. Nearly all of these were connected with the control of the amount of preview permitted and with obtaining a meaningful record of the subjects' activity. In the main he had used tasks in which the display changed independently of the subject's response. Making use of apparatus in which the presentation of the next signal was dependent upon the subject first making a correct response went a long way towards solving those difficulties in the experiments to be reported. Originally the problem appeared to be how to present advance information adequately; how to prevent subjects from obtaining advance information proved eventually, however, to be the real problem.

The purpose of the experiments may now be considered in more detail. Consider a line A–F. A subject starting at A is given a signal to go to B, on arriving there a signal to go to C, and so on. This may be considered to be the basic situation. How

would it affect his performance if we were able to tell him to go to C when he gets to a (say halfway between A and B), to D when he gets to b, and so on? Will he take less time to get from A to F, and if so how will this saving be achieved? Even if there is little or no difference in his over-all time, will there be a difference in his pattern of activity?

The methods and the results of each experiment will be presented separately. This will be followed by a discussion of both experiments.

II

Apparatus

Two pieces of apparatus were built. Since the circuits for both were somewhat complicated it is intended to refer only to a few important points about them. In both cases post-office uniselectors provided the stimulus sequences, these being wired up permanently but capable of permutation by means of switches. At any given moment one of a number of stimulus relays would be energized. This would cause a neon bulb to strike and a path to be established between the corresponding response point and the "correct" relay. Current being supplied to the response point would cause the correct relay to operate and the selector to move on by one step. In both pieces of apparatus equipment was provided for stopping the sequence when a given number of responses had been made by the subject. Recording was carried out in terms of time per trial, to the nearest second, obtained by a counter driven by a ½-second clock, and component records by means of on-off pens for various single components per response, to 0·05 secs. in Experiment I, and to 0·025 secs. in Experiment II. All equipment apart from control and display was kept outside the experimental room. Whitfield's (1947) method was used for statistical tests of significance.

The display of the *stylus* apparatus consisted of 5 neon-lamps forming a pentagon with 3½-in. sides. This was placed in the vertical plane about 22 in. to 24 in. away from the subject's eyes. The control consisted of a flat panel with brass discs 1¼ in. diam. let in flush. Five discs formed a pentagon corresponding to that of the display, and one disc was located in the centre. The subject held in his hand a stylus with a brass tip and wire attached. The task could be given under two conditions: (1) starting from the centre disc the subject had to slide the stylus to the disc indicated by the neon-lamp which was lit and then to slide back to the centre disc. The display did not change to one of the other four lamps until he had got to the centre disc. (2) Starting also from the centre disc, the display was changed as soon as the subject made contact with the indicated disc. He had then to touch the centre disc before proceeding to the indicated disc.

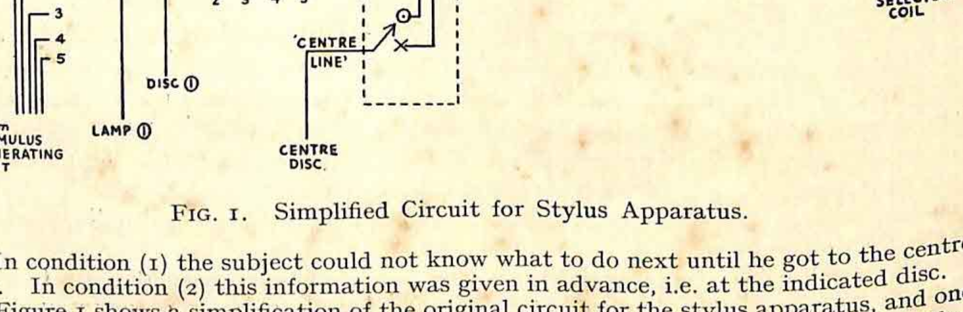

Fig. 1. Simplified Circuit for Stylus Apparatus.

In condition (1) the subject could not know what to do next until he got to the centre disc. In condition (2) this information was given in advance, i.e. at the indicated disc.

Figure 1 shows a simplification of the original circuit for the stylus apparatus, and one used in a later model. There are no doubt many ways of achieving most of the other

requirements of this apparatus, but it is thought that the manner in which the two conditions were produced is perhaps the simplest possible. This depended on the fact that each response could be considered as having two stages: one beginning at the centre disc, and the other at the indicated disc. On the right in Figure I are two relays marked intermediate and correct. The intermediate relay had to be closed before the correct relay could be energized, and this in turn caused the selector to move on by one step and the display to change. The two conditions were produced by allowing these two relays to be energized from different positions of the control. In condition (1) contact on the indicated disc would close the intermediate relay and subsequent contact on the centre disc would close the correct relay. In condition (2) this arrangement was reversed. Figure I also shows one of the stimulus relays, the origin of the "correct" line, the switching circuit, and the two relays already referred to. The double change-over switch is shown as set for condition (1).

In the *joystick* apparatus there was a primary display consisting of three neon-lamps arranged at the corners of an equilateral triangle with sides $3\frac{1}{2}$ in. At a distance of 1 in. away from each of these lamps and along the sides of the triangle there were six further lamps, two to each corner. Each of these lamps was surrounded by an arrow pointing towards the other corners of the triangle. The display panel was at right angles to the subject's line of vision and about $4\frac{1}{2}$ ft. away from him. The control consisted of a joystick having a guide-arrangement which allowed movement to be made only along the sides of an equilateral triangle having sides of $3\frac{1}{2}$ in., the handle of this joystick projecting 6 in. from this guide at mid-point, so that a movement of about $8\frac{1}{2}$ in. was required between any two corners of the triangle. The control was fixed to a heavy base on the floor, the top of the joystick being about 2 ft. 7 in. from the floor. The subject sat with the control between his legs.

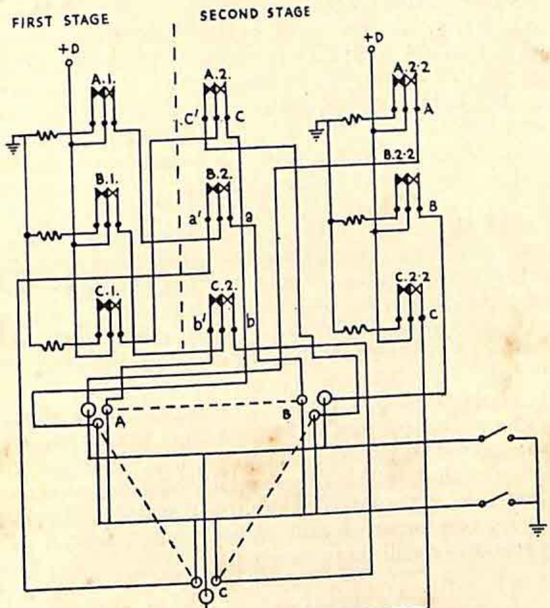

FIG. 2. Circuit for Display of Joystick Apparatus,

In this apparatus advance information was presented by an addition to the main display, i.e. the secondary display consisting of the arrows. In terms of circuit, this required that there should be two stages of stimulus relays. This was achieved by having three sets of progressive chain circuits, having two relays each. The selector would energize the first stage directly, and this would hand on to the second stage every time a correct response was made. The subject had to respond according to the indication given by the second stage, i.e. the primary display; whilst the first stage indicated the state of the second stage "one ahead." Figure II shows the display circuit. The advance indicators used here were possible because firstly one was dealing with three positions only,

and secondly no signal could occur twice in immediate succession: any one signal could therefore be followed by only one of the other two.

The primary display lamps A, B, C, were lit by a relay of the second stage. This requires no further explanation.

The secondary display lamps a′, a, b′, b, c′, c were made by contacts on first stage relays, the leads being taken to the commons of change-overs, on second stage relays, i.e., from relay A1 to B2, from B1 to C2, and from C1 to A2. With relays A1 and B2 energized lamps B and a would be on indicating that the joystick should be moved to corner B first, then to A.

In condition A only the primary display was used, whilst in condition B the secondary display was switched on as well.

III

Experiment I

This was carried out with the stylus apparatus. There were two groups of subjects, there being five male, and five female in each group. Each subject was given two trials of 100 responses each under one condition only. Two separate unmatched groups were used because pilot runs had indicated adverse interactions between the conditions. A more complete check was carried out later and fully justified this design. Subjects had the control-display relationship explained to them, were asked never to lift the stylus off the panel, and were told that their task was to make 100 correct responses in as little time as possible, making as few errors as possible. They were told that the apparatus would cut off when they had completed a trial. Possible errors were demonstrated, and subjects were allowed to make a few practice moves before the first trial (mainly to show the experimenter that the instructions had been understood). They were not given any specific instructions on how to do the task. Time per trial was given to the subjects after each trial. Apart from the general instructions there were differential instructions for the two conditions: (1) "Starting from the centre, slide the stylus to the indicated disc and come back to the centre when one of the other four lights will come on. Only one lamp will be on at any one time." (2) "Starting from the centre, slide the stylus to the indicated disc when one of the other four lights will come on. You must touch the centre disc before going on to the next indicated disc. Only one lamp will be on at any one time."

It will be noted that there is a difference between these two sets of instructions. Ideally one would have liked the instructions for condition (2) to be more like those for condition (1), i.e. mentioning the return to the centre before the change of display. This had lead to considerable confusion in pilot runs, presumably because the temporal order of events as given in the instructions did not fit with the actual order of events.

Apart from total time per trial the following component times were obtained for each response:

Time spent on the centre disc (C), time spent between centre disc and indicated disc (Out), time spent on the indicated disc (EP) and time spent between indicated disc and centre disc (In). Actual error time was also scored, but, since there were very few errors, this time was lumped together with irregularities of the record to provide a residual amounting to rather less than 5 per cent. of the total recorded time under both conditions. The first and the last 25 responses of each subject were scored in detail, and only the results for the last 25 responses will be presented.

Results

TABLE I

Mean Times per response in seconds

	First Trial	*Second Trial*
Condition 1	1·09	0·95
Condition 2	0·90	0·75
	0·19	0·20

Both differences are significant at better than 1 per cent.

It should be noted that subjects performing under conditions (2) took less time per response in both trials.

TABLE II

COMPONENT ANALYSIS FOR LAST 25 RESPONSES

	Centre	*Out*	EP	*In*	*Residual*	*Total*
Condition 1 ..	0·39	0·15	0·20	0·11	0·04	0·89
Condition 2 ..	0·19	0·17	0·19	0·14	0·03	0·72

Time in Seconds.

The component analysis (Table II) showed that this difference was due to less time being spent on the centre disc ($P < 0{\cdot}001$) under condition (2), whilst time spent over the other components was little affected. There was some suggestion that if anything movement times were rather longer under condition (2). From the data it seems reasonable to infer that in condition (2) action was initiated well before the centre disc was reached.

It was observed that under condition (1) there was only one effective method of performing, i.e. to move out to the indicated disc, return, and then stop at, or at the most "dither" about on the centre disc. Under condition (2) one could perform in one of three ways: firstly rather as under condition (1), i.e. the emphasis on the centre disc, secondly by stopping on the indicated disc and then moving straight through the centre disc, i.e. the emphasis on the indicated discs, and thirdly, by trying to keep moving more or less the whole time. There was some tendency for this last method to be adopted by most subjects towards the end of the experiment.

IV

EXPERIMENT II

This was carried out with the joystick apparatus. It seemed desirable to demonstrate the phenomenon of receptor-effector anticipation in a rather more constrained setting in which the advance information could be presented explicitly rather than implicitly and where recording would be rather less ambiguous than it had to be inevitably with the unconstrained type of control. In particular it was thought important to follow up the hint about longer movement times obtained in condition (2) of the first experiment.

TABLE III

EXPERIMENTAL PLAN FOR EXPERIMENT II

Group		*1st Day*	*2nd Day*	*3rd Day*
	Trials	1–4	5–8	9–11
Control	*Conditions*	AAAA	AAAA	AAB
Experimental..			AABB	BBA

Table III shows the experimental plan. There were 10 naval subjects in each of two groups. They were matched on time and error scores obtained from their first four trials. The experiment could be given under the two conditions already described in the apparatus section. The subjects' task was to make 99 correct responses per trial in as short a time and with as few errors as possible. An error was scored for any entry,

however short into an alley leading to the not-indicated corner. Since there was only very little movement tolerance at the corners of the control, this task was not as easy as it might appear, and this was pointed out to the subjects. They were instructed to pay attention to accuracy and to speed up performance only when they felt sure about the manipulation of the control. Subjects were told their time and error scores after each trial. Condition A was explained to them before the first trial and the experimenter demonstrated a few responses at a slow rate. Condition B was explained to the subjects only before the first trial under which they were to be exposed to it. In this case the experimenter did not demonstrate again.

Scoring was in terms of Total Time per Trial, Errors per Trial, and component times for each response:

(1) Time spent at the correct end-point (EP).
(2) Time spent between leaving end-point and entering correct path (OUT) (for technical reasons it was necessary to have at each corner a very small area common to both paths meeting at the corner, and the above component refers to this).
(3) Time spent between entering correct path and reaching indicated corner, i.e. movement time (M).
(4) Time spent over errors. Error time was all the time between entering a wrong alley and entering the correct one.

Results.

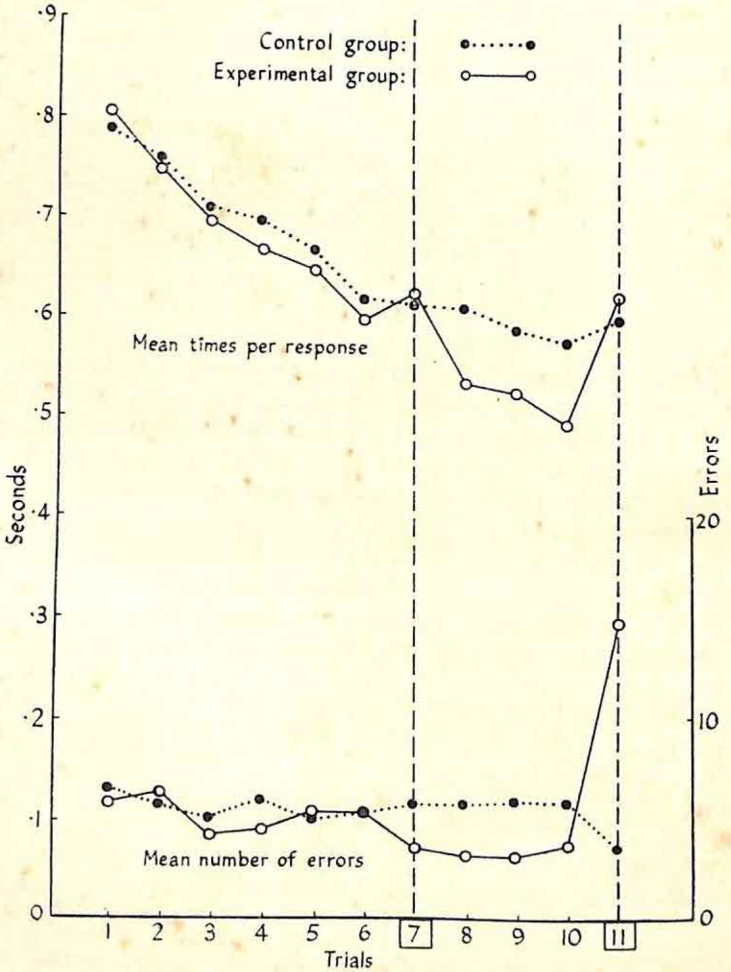

FIG. 3. Practice Curves for Experiment 2.

Figure 3 shows the practice curves obtained in terms of response time and errors per trial. It is of some interest to note that the introduction of advance information produced very similar immediate effects for both the experimental and the control groups: times went up and errors came down. Also noteworthy was the quite unexpected effect on the error-score when the experimental group was once more given condition A on the last trial.

TABLE IV

COMPONENT ANALYSIS FOR 8TH TRIAL

Group	*EP*	*Out*	*M*	*E*	*T.R.T.*
Control	0·269	0·051	0·229	0·028	0·577
Experimental	0·180	0·052	0·258	0·022	0·512

Time in seconds.

The eighth trial was chosen for detailed analysis and the results are presented in Table IV. TRT stands for Total Recorded Time. Here, too, there was a reduction in the stopping time ($p < 0{\cdot}001$); the movement times were slower, but the difference was not significant. The opposite trends for stopping and movement times were shown rather more markedly in the values obtained for the five fastest subjects in each group as shown in Table V.

TABLE V

COMPONENTS FOR FIVE BEST SUBJECTS IN EACH GROUP

Group/Component	*EP*	*O*	*M*	*E*	*TRT*
Control	0·268	0·046	0·196	0·021	0·531
Experimental	0·147	0·050	0·265	0·018	0·480
Difference	0·121	−0·004	−0·069	0·003	0·051

Here something like half the reduction in time gained by short stopping times was cancelled out by the longer movement times.

There seems to be a perfectly straightforward explanation for the longer movement times. It will be seen that particularly with the faster subjects response rates were very close to two responses per second for both conditions. This rate has been held to constitute something of a limit in comparable serial tasks (Hick and Bates, 1950). Since the rate of stimulus presentation was determined by the rate at which subjects made correct responses, the faster they responded the faster signals would be generated. If it may be accepted that subjects were trying to fulfil instructions and operate as fast as possible it will be seen that in condition A, with an enforced stopping time at the indicated corner, the only way to reach the limit was to move fast. In condition B, i.e. with advance information, this stopping time could be reduced but, had subjects also moved fast, they would have generated signals at too high a rate. This illustrates once again that an overall difference is likely to be a resultant of more than one trend rather than the direct reflexion of one affected component.

The fact that the two-a-second limit seemed to operate even under condition B suggests that any more than "one signal ahead" could not raise the response rate.

In fact subjects tended to report that with one item ahead they were kept fully occupied.

In this experiment, when advance information was given, subjects often reported an uncanny lack of awareness of what they were doing, although they were plainly doing the right thing. This experience was by no means entirely pleasurable. In the words of one subject: "without the arrow lights you feel you are chasing the lights; with them the lights are chasing you."

V

Discussion

In the two sections concerned with results the emphasis was deliberately placed on the quantitative rather than the qualitative aspects of the two experiments. It is quite evident that the measurable effect of providing subjects with advance information was a higher response rate resulting from shorter stopping times. It is clear that with advance information the two operations of "choosing" and "moving" could overlap. It would also seem that such an overlap by itself did not raise response rates markedly above two a second. That is to say, the kind of advance information provided did not release one from those constraints of discrete manual responding which have now been known for some time. These findings are naturally of some importance but they cannot convey anything like the whole picture.

Far more important than the effects on response rate were the differences in the patterns of performance under the two conditions. These differences are of course reflected in the component analyses but it must be admitted that to appreciate them fully they have to be seen, or better still experienced. Without advance information subjects were forced into momentary, overt inactivity during the "choice-time," and seemed to compensate by fast movement. When advance information was available, they tended to perform more smoothly, with one response easily merging into the next. This difference, striking already in the stylus experiment, was even more marked with the joystick apparatus. The reason for this was almost certainly that, with this apparatus more than with any other, one was able to observe the full effects of not having advance information. Because of an almost complete absence of free-movement tolerance at the end-points subjects had to come to a complete stop —and this was not merely an absence of movement but a controlled activity requiring a measure of skill. For in these conditions—naively thought of as control conditions —subjects were placed into a succession of conflict situations: they knew that as quickly as possible after they had completed one response they ought to be on the move again, and in one of a limited number of directions. But they also knew that in order to do so adequately they would have to come to a stop whilst selecting one direction according to the display state. The conflict showed itself in rigid or ragged performance, for the number of possibilities being strictly limited, and the task being of a sequential kind, the tendency to guess was considerable and had to be inhibited. The very real sense of freedom imparted by them being provided with advance information was remarkable. In the case of the joystick subjects not infrequently suspected the experimenter of having "done something" to the control box as well as to the display.

The above description in terms of conflict may also help to clarify the extraordinary incidence of errors on the experimental group's last trial, i.e. when subjects were once more performing without advance information. It seems reasonable to think that earlier on they had acquired a measure of control over the conflict situation. The experimental situation resolved the conflict, but since neither conflict nor resolution had ever become formalized, they tried to transfer the easy smoothness of

performance under condition B to condition A. The simplest way to describe their behaviour then is to say that they were "caught out."

It is clear, upon reflexion, that most everyday situations are comparable to those conditions in the experiments in which advance information was provided, i.e. one manages to avoid the conflict by being able to provide guidance continuously, by taking in information before the point of action is reached. At any rate, one tries to do so.

On the relation between receptor-effector anticipation and other forms of anticipation, only little need be said here. Receptor-effector anticipation depends on the presence of a signal which can be observed and acted upon ahead of the action point. The other forms depend on past experience. The weakness of anticipation based on past experience lies in the fact that in so many situations it cannot by itself fully prepare responses, it is not enough to provide complete certainty because very often there is a residue of uncertainty, a moment to moment randomness in the environment. Anticipation here must serve to limit the possibilities. The weakness of anticipation based on information present in the environment is its dependance on this information which may be "coded" or embedded in a host of other information. Together they seem to overcome each other's disadvantages. The practice curves obtained in the joystick experiment may serve to illustrate these points. There was a speeding up of performance merely as the result of practice until something like a levelling out began at the sixth trial, and any further speeding up under those conditions would presumably have taken very much more practice. When advance information was presented there was at first a slight slowing up, presumably accounted for by the new perceptual situation, but from the eighth trial onwards there was a clear breakaway by the experimental group.

In comparing the two experiments, it should be noted that in the stylus experiment advance information could be presented implicitly, whilst the presentation in the joystick experiment was of an explicit kind. That is to say, there was no need to refer to advance information, or to the different meaning of the display under the two conditions used in the stylus experiment, whilst in the joystick experiment such explanation had to be given. Since it is also the simpler of the two pieces of apparatus, the stylus apparatus is to be preferred for demonstration purposes.

The investigation of receptor-effector anticipation was suggested by Sir Frederic Bartlett. The work was carried out at the Psychological Laboratory, University of Cambridge, under the general supervision of Mr. A. T. Welford, with grants from the Rockefeller Foundation and the Ministry of Education.

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FIXATIONS, POSITION STEREOTYPES AND THEIR RELATION TO THE DEGREE AND PATTERN OF STRESS
PART II

BY

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In a group of white rats who developed position stereotypes in a water discrimination unit under conditions of low punishment, the behaviour of four animals was closely akin to what Maier calls "abnormal fixations." The stereotype-extinction scores of subgroups containing these animals were distributed in separated clusters. Punishment differentiated response latencies, and this furnished evidence that the fixated animals had mastered the soluble problem in spite of not responding appropriately.

These findings and other findings from the same experiment, which were reported previously (Knöpfelmacher 1953), are interpreted as showing that though Maierian behaviour fixations were reproduced in a water discrimination unit, the properties of stereotypes are not compatible with the *stress—frustration—fixation* theory, at least on a molar level, and that *goal—motivational* learning theory can account for most though not all the data.

I
Introduction

In a previous report (Knöpfelmacher, 1953) we described an experiment which tests the hypothesis that the strength of behaviour stereotypes elicited in an insoluble problem situation is a function of stress. Forty-six white rats were split into two groups, I and II, and were given an insoluble problem in a water discrimination unit. The two groups were trained under conditions of low and high punishment respectively. Those animals who developed position stereotypes were assigned to two subgroups, a and b, within each group, and were given a soluble discrimination problem under conditions of 50 and 100 per cent. punishment respectively. The strength of stereotype was measured in terms of stereotype breaking scores. It was found that animals trained under conditions of low punishment, i.e. groups Ia and Ib developed the more persistent stereotypes. Animals trained under conditions of 100 per cent. punishment, i.e. groups Ib and IIb developed the less persistent stereotypes and the longer spans between stereotype-breaking scores and the learning criterion. Groups Ia and Ib included four animals who did not abandon their stereotyped responses at all even when punishment was increased and who showed some other characteristics associated with "fixation" as described by Maier (e.g. Maier, 1949). In the next section we give a detailed qualitative description of the behaviour of these four animals. The discussion and bibliography which end this report relate not only to findings reported below but equally to those reported previously (Knöpfelmacher, 1953).

II
Qualitative Description of the Behaviour of Fixated Rats

Four animals—three in group Ia and one in group Ib—did not reach the breaking criterion in phase 4 and had to be advanced to phase 5 (Knöpfelmacher, 1953, Tables I and II). The trend of their response latencies indicated that they have mastered all information provided by differential cues. Their deficiency in behaviour plasticity could therefore not be ascribed to deficiency in learning at least without important

speculative qualifications. In addition to what are clearly quantitative criteria of fixation, these animals exhibited traits which we did not quantify but which are worth reporting, since they confirm some of Maier's findings and differentiate the animals from the rest of the population sample. One animal in group IIa though it did not meet all quantitative criteria of fixation, was closely similar to fixated animals in many other respects. Its description will therefore be included under the present heading.

No. 13. The behaviour of this animal in phase 4 was distinguished mainly by a strikingly steep increase in response latencies, which, though undergoing considerable fluctuations from trial to trial, were always much above the group average (Knöpfelmacher, 1953, Figure XII). On the first day of training under increased punishment, the animal repeatedly failed to enter the detention compartment with its whole body, though it made the choice by sticking its head through the door. From the fourth trial onwards the animal struggled whenever the experimenter tried to put it into the apparatus. From the 15th trial onwards, the animal swam repeatedly to the positive door, sniffed at it and hovered in front of it for increasingly long periods in a freezing posture which it terminated abruptly, by suddenly turning round, retracing to the starting point and swimming through the negative door into the detention compartment. Frequently the animal merely retraced to the starting point and returned to the positive door to renew its freezing posture. After the 52nd trial it began to show signs of freezing behaviour at the starting point, its tendency to swim to the positive door decreased markedly and degenerated into vicarious trial-and-error behaviour at the choice point, biased towards the positive door. This was the closest approximation we obtained to the "longing look" towards the positive door, observed among fixated rats by Maier. The longest response latencies occurred between the 70th and the 80th trial; they were almost entirely due to time spent at the starting point in a freezing posture, with the head turned away from the choice point. It looked as if the animal were trying to "avoid" the choice point. After the 90th trial the freezing behaviour at the starting point disappeared and the rat spent most of its pre-response time in front of the negative door in a freezing posture. This posture was usually terminated by a sudden thrust of the head through the door. The rest of the animal's body had to be pushed through into the detention compartment by the experimenter. This pattern of behaviour was continued up to the end of phase 4. In phase 5 the animal resisted guidance violently and struggled with and bit into the wooden spoon which the experimenter used for this purpose. After six guided trials the animal swam spontaneously and without any vicarious trial and error through the positive door. It failed, however, to alternate from side to side when the positive door was being shifted in random order and thus adopted a new position habit, opposite from the one it had previously fixated. After swimming persistently to the newly habituated side for a whole day's trial series it was again subjected to guidance. After nine guided trials the new position habit was abandoned. It is interesting to note that the number of trials required to break the post-fixation position habit by guidance was higher than the corresponding number of trials required to break the fixation.

Nos. 6, 9 and 34. The behaviour of these animals was less strikingly different from the rest of their group than the behaviour of rat No. 13 from the rest of its group. Inspection of Fig. VI (Knöpfelmacher, 1953) shows that they reached a level of response latencies which was apparently not different from the corresponding level reached by animals who broke their stereotypes. All three rats showed marked vicarious trial-and-error behaviour at the time when their response latencies began to increase. After this they swam repeatedly to the positive door first, turned round and swam through the negative door into the detention compartment. There were no signs of resistance to responding similar to those observed in No. 13, through its freezing behaviour at the starting point. There was some resistance to guidance, though much less violent than in the case of rat No. 13. The animals resisted the experimenter's attempts to put them into the apparatus quite frequently and squealed profusely—particularly rat No. 6. Their behaviour was homogeneous in all other respects.

No. 22. This animal, though not satisfying the main quantitative criterion of fixation—it broke the sterotype without guidance—resembled rat No. 13 in some other respects. It reached the highest level of response latency observed in this experiment—756 seconds. After the tenth trial in phase 4 the animal began to swim to the positive door first, before entering the detention compartment through the negative door. After the 17th trial,

resistance to respond became manifest in that the animal spent most of its pre-response time at the starting point in a frozen posture. On a number of occasions the animal went to the positive door first and then retraced to the starting point, where it remained in a frozen posture for several minutes, before swimming through the negative door into the detention compartment. After 43 trials it abandoned the position stereotype.

III

Discussion

The results of our experiment count against the hypothesis that the strength of position stereotypes is a positive function of stress, in every respect. Rats trained under conditions of high punishment developed position stereotypes far less rigid than were those developed under conditions of low punishment. The four fixated animals belonged to groups Ia and Ib and not to groups IIa or IIb as would be expected if there were a functional relation between fixation and stress. Increase in punishment not only failed to impede but precipitated breaking of the stereotypes. Animals presented with the soluble problem under conditions of 50 per cent. punishment took longer to reach the breaking criterion than animals trained under conditions of 100 per cent. punishment. There was no significant difference in proportion of fixations between groups a and b; the difference was actually in a direction opposite from the one expected by Maier, since there were three fixated rats in group Ia and only one in group Ib.

Attempts have recently been made to explain rigid position stereotypes in terms of a learning theory, extended to include the working of secondary drives. Mowrer (e.g. 1950) suggests that position stereotypes may be motivated by fear reduction at the choice point. Fear is generated by the stress of an insoluble problem and conditioned to the cues at the choice point. A stereotyped response, by moving the animal away from these cues, reduces anxiety and is thereby reinforced. Farber (1948) claims to have confirmed this hypothesis by showing that animals trained to make a discrimination response under conditions of fear at the choice point, achieve habit reversal with less difficulty, if the fear-conditioned stimuli at the choice point have been associated with reward before reversal training is started. By dissipating anxiety Farber believes he has reduced the rigidity of an allegedly anxiety motivated habit. Our results do not lend support to this hypothesis. If the position stereotypes were motivated by anxiety reduction at the choice point, there would be some reason to expect more rigid stereotypes among animals trained under conditions of heavy punishment and not the exact opposite, as happened in this experiment. Though not supporting Mowrer's hypothesis, our results cannot be interpreted as contradicting it, mainly for two reasons.

Firstly, there is very little experimental evidence that the strength of secondary drives is a positive function of the strength of primary drives. It cannot be claimed that such a functional relationship is warranted by results obtained by Pavlov and those who used his technique. It is true that these results show that the strength of the conditioned response is *inter alia* a function of the strength of the unconditioned response, but this finding by itself does not warrant the deduction, that therefore the strength of secondary drives must be a positive function of the strength of primary drives. Mowrer's hypothesis that fear is a conditioned response syndrome, elicited by Pavlovian conditioning and reinforced by contiguity with pain is a *hypothesis* which requires independent evidence. Unless that evidence is forthcoming the working of Pavlovian principles with respect to the connection between primary and secondary drives cannot be taken for granted. Only if it were shown that the functional relation between primary and secondary drives is isomorphic

with the functional relation between conditioned and unconditioned responses, could more reliance be placed on arguments by analogy from Pavlovian conditioning. Miller (1951) reports results which indicate that secondary drive strength may be a positive function of primary drive strength. Some results obtained by Gwinn (1951) could also be interpreted as showing somewhat inconclusively that secondary drive strength varies positively with primary drive strength. In an experiment by Hurwitz (1953) two groups of rats were trained to cross an electrified grid under conditions similar to those which were reported by Mowrer (1950) as having been used by Whiteis. The groups were motivated by differential intensities of shock. The Whiteis phenomenon was reproduced more frequently in the low shock group. This result does not support the hypothesis that the strength of secondary drives is a positive function of the strength of primary drives. Hence it clearly cannot be taken for granted that longer detention leads to more anxiety at the choice point.

Secondly, the assumption made, e.g. by Finan (1940) that the strength of a reaction depends on the level of motivation and strength of reinforcement under which it was acquired, is open to doubt in view of evidence to the contrary reported by Kendler (1945), Strassburger (1950), Reynolds (1950), Reynolds, Marx, and Henderson (1952) and Teel (1952). These experimenters have obtained results which indicate that resistance to extinction does not increase positively with magnitude of drive or reward in the acquisition phase. If we generalize the hypothesis supported by these findings to cover phenomena of anxiety learning, it would imply that the resistance to extinction of an anxiety-motivated response is independent both of the degree of anxiety and the amount of anxiety reduction in the response-acquisition phase.

In a paper published after Mowrer's and Farber's criticism of Maier's work, Maier and Ellen (1951) try to show that the stress-frustration-fixation theory can give a more adequate account not only of Maier's own experimental results, but also of findings which underlie the theory of Mowrer. They do this by postulating that e.g. the rigid running habit elicited in the Whiteis apparatus is frustration-instigated. Farber's results are explained by assuming that reward at the choice point dissipates frustration. Since in each case a hazily conceived frustration mechanism is postulated *post hoc* these explanations are not very helpful. On the other hand Maier and Ellen's further argument that an anxiety-reduction theory cannot explain the bimodality of the distribution of stereotype extinction scores, that it cannot explain the absence of behaviour variability under conditions of an insoluble problem and the striking strength of fixated habits, seems to be unanswerable. It applies to our results as well, since breaking scores of the group containing the fixated animals were not unimodally distributed and the behaviour of fixated animals showed a great lack of variability and in some cases remarkable persistence.

In speculations which led to hypotheses deducible from the stress-frustration-fixation theory or from the anxiety-learning theory, the possibility that escape from the noxious stimulus itself may reinforce the stereotype is never considered. According to the former theory no reinforcement at all occurs, since the sterotype is frustration-instigated. The hypothesis deduced from the latter theory implies that reinforcement occurs in close temporal contiguity with the response and that it cannot be generated by the unsuccessful termination of the response. Since neither of the two explanations is compatible with our results we suggest a hypothesis based on the assumption that the sterotype may be reinforced by escape from the water: escape from the detention compartment may be looked upon as the goal which terminates the sequence of locomotor responses in the apparatus. The effect of escape on responses preceding it will vary with the time interval between

them; the corresponding effect of detention will vary with the duration of detention. Escape reinforces approach behaviour towards the escape ladder; detention, interposed between the animal and the escape ladder, weakens it. Since the effect of reinforcement on a response is a negative function of the time interval between them, position stereotypes are more effectively reinforced under conditions of 8- rather than 80-seconds detention. The negative effect of detention will on the other hand be stronger under conditions of 80-seconds detention. An independent indication of this has been obtained through the analysis of the distribution of escape reactions reported in section III (Knöpfelmacher, 1953). It follows that the effects of both escape and detention favour the formation of stronger position stereotypes in groups Ia and Ib rather than in groups IIa and IIb. Expressed anthropomorphically, groups IIa and IIb face longer periods of detention and more distant reward and are therefore less keen to respond. This alone cannot, however, explain the *complete* failure of most animals in groups Ia and Ib to abandon the stereotypes in phase 3. An alternative hypothesis, not necessarily incompatible with the one enunciated above is this: reinforcement in phase 3, consequent upon a response to the positive door, may not have been sufficiently different from what was punishment consequent upon a response to the negative door to motivate discrimination learning. If differential motivation was absent, the two signs to which differential amounts of reward were attached, did not represent cues of differential incentive value to the animal. It would then follow, that "from the animal's point of view" all responses in phases 2 and 3 were equally rewarded.

This leaves us with the problem of the four (if we exclude No. 22) fixated rats. It cannot be held that failure to break the stereotype in phase 4 is due to the same factors as the corresponding failure in phase 3. The changes in behaviour which were observed among the fixated animals only when they were started on phase 4, and which were not observed during any previous experimental period on any animal, make such an assumption untenable. The behaviour pattern of the four animals is analogous to the behaviour pattern of Maier's fixated rats by virtue of its following characteristics:

(1) The stereotype did not yield to extinction by consistent reward and punishment.
(2) There was evidence of latent learning in that response latencies were appropriately modified when the problem was made soluble.
(3) The distribution of breaking scores of the groups containing the fixated animals were not unimodal.
(4) There was a tendency to turn towards the positive door while responding to the negative one. The animals swam towards the positive door before choosing the negative one and displayed vicarious trial-and-error behaviour clearly biased towards the positive door.
(5) The fixated stereotypes were broken by guidance.
(6) Vigorous resistance to respond was developed in some cases, as shown, e.g. by freezing behaviour at the starting point.

It can be concluded that the outward behavioural manifestations of what Maier calls "abnormal fixation" were fairly closely reproduced in this experiment. However, the relationships between fixations and the independent variables of the experiment—degree and patterning of stress—were quite incompatible with Maier's stress-frustration-fixation theory. Are the fixations explicable in terms of goal-motivational learning theory? It may not be impossible that they were due to overlearning. All fixations occurred in groups Ia and Ib; if our hypothesis concerning the absence

of differential motivation of these groups were correct, all their responses throughout phases 2 and 3 would have to be regarded as equally reinforced. Our analysis of the effects of escape and detention would also lend support to the hypothesis that overlearning causes fixation. There are some experimental findings, however, which do not lend support to this hypothesis: Differentiation of response latencies to the positive and negative doors, both among the fixated and among the non-fixated animals, indicates that with respect to certain manifestations of discrimination learning the two sets of animals were homogeneous. Fixated animals frequently approach the positive door, though they do not swim through it. They do so early in the trial series. Why are they unable to learn just the final segment of the positive response after many more trials, if they have mastered most of its segments without the slightest difficulty? Why does overlearning of an old habit interfere with the learning of just one particular constituent of a chain of locomotor responses? Growing resistance to respond, resistance to guidance, struggling and squealing, all these phenomena appear to be equally inexplicable in terms of overlearning alone. It must therefore be concluded that the explanation of fixations in terms of overlearning accounts for only a part of the observed phenomena and is consequently not very satisfactory.

All hypotheses so far considered were in some way connected with the dynamics of motivation. Eglash (1951) claims that it is possible to explain fixations as the result of purely cognitive factors, in accordance with a suggestion made by Hilgard (1948). He argues that animals who were presented by Maier with a symbol-reward problem, were in fact confronted with a number of separate cognitive tasks. They had to perceive the shift from 50 per cent. random to 50 per cent. orderly punishment and they had separately to associate (*a*) the positive card with reward on the habitual side, (*b*) the positive card with reward on the non-habitual side, (*c*) the negative card with punishment on the habitual side, and (*d*) the negative card with punishment on the non-habitual side. The differences between Maier's experimental and control groups are interpreted as arising out of different complexities of task which differential shifts in cues imply. The experimental and control groups are in fact confronted with perceptual, association and thinking problems of different degrees of difficulty, and their adaptability varies accordingly. Eglash's suggestion agrees with our own in so far as he attempts to explain sterotypes without the hypotheses that a change from primary to secondary or to no motivation has occurred. The following is an attempt to establish how far our results are explicable in terms of his hypothesis.

1. The difference between breaking scores of groups I and II. Groups Ia and IIa are shifted from a random to a regular pattern of punishment. Both groups have to notice this change by associating the illuminated and the dark doors on the habitual and non-habitual side with escape and detention respectively. There does not seem to be any cognitive factor in the situation which would favour group IIa as against group Ia, unless we assume that heavier punishment provides additional cues. To assume this, we would have to extend the cognitive hypothesis to include the function of drive intensity as a special vehicle of additional information, which could be done only on purely speculative grounds. The arguments used with respect to groups Ia and IIa apply analogously to the difference between groups Ib and IIb which also appear to have been presented with tasks of equivalent difficulty. It seems that the difference in stereotype rigidity between groups I and II is inexplicable in terms of Eglash's cognitive hypothesis.

2. The difference between breaking scores of groups a and b. There are differences in perceptual cues and associative complexity which favour groups b as against

groups a. This applies equally to groups IIa and IIb in phase 3 and to groups Ia and Ib in phase 4. The shift from random to consistent 50 per cent. punishment offers less cues than a shift from 50 per cent. random to 100 per cent. consistent punishment. To shift from the habitual side groups b have to associate it with punishment. Groups a have to solve the more complicated problem of associating light on the habitual side with punishment and darkness on the habitual side with reward, since their habitual side is not consistently punished. Hence the difference between groups a and b accords with the cognitive hypothesis.

3. Differences in B–L spans. Though only groups IIa and IIb were significantly differentiated by their B–L spans, the following argument applies equally to groups Ia and Ib in phase 4 as their results pointed in the same direction. To master the soluble problem, the animals must associate the positive and negative signs on both sides with reward and punishment respectively. Groups a experience the sequence light-punishment and darkness-reward on their habitual side prior to the attainment of the breaking criterion; they have to infer that the two signs are associated with reward and punishment in the same manner on the non-habitual side as well. Groups b experience merely the darkness punishment sequence on the habitual side. They are never presented with the positive sign on the habitual side and can form neither the association between it and reward, nor the additional inference that this association holds for both sides, prior to the attainment of the breaking criterion after which treatment of groups a and b is the same. Groups b can therefore form most of the associations which contribute to the solution of the discrimination problem only after they had abandoned the position stereotypes. Moreover since breaking of the stereotype can be achieved by merely associating the habitual *side* with punishment, it is quite possible that groups b do not form any association with illumination before the breaking of sterotype. It follows that the longer B–L spans in that group are clearly to be expected on the basis of the cognitive hypothesis.

4. Fixations. A satisfactory explanation of stereotype fixations would have to cover the non-unimodal distribution of stereotype rigidity, the differentiation of response latencies without subsequent shift of response, the change in direction of locomotion consistent with the entire appropriate response short of its final most important segment, and it would have to cover also the qualitative peculiarities of the fixated animals, viz. struggling, squealing and resistance to guidance. It could be argued that the strength of stereotypes is not unimodally distributed because the appropriate cognitive abilities are not unimodally distributed. Such hypothesis would be very unsatisfactory in view of the purely speculative character of the assumption about the distribution of cognitive abilities and also because it fails to explain why this alleged distribution does not lead to analogous consequences in groups IIa and IIb. The change in response latencies could be explained by assuming that it marks the animal's awareness of the punitive consequences associated with the negative sign on the habitual side and merely signifies its increasing reluctance to swim through the negative door. A shift of habit requires the additional association of the positive sign with reward on the non-habitual side or merely the association of the non-habitual side with reward, which some animals may be unable to make. Hence the change in response latencies could be interpreted as pointing to a differentiation of resistance to certain determinate cue-configurations, which are not all the cue-configurations required for learning the soluble problem or the breaking of stereotype. The above argument breaks down, however, as soon as we consider the animal's "unfinished" responses to the positive door. These indicate that the animals have formed the appropriate associations, made the necessary inferences and are still not able to abandon their stereotyped responses. Resistance to respond-

ing and guidance, squealing and struggling are qualitative peculiarities of fixated animals which are quite inexplicable in terms of a purely cognitive hypothesis. We have to conclude that this hypothesis is not adequate for explaining the occurrence of stereotype fixations in this experiment.

The outcome of the above analysis does not encourage acceptance of Eglash's cognitive hypothesis. Only two out of four main results are explicable in its terms. It appears that our own hypotheses based on primary motivation, on the interaction of approach and avoidance gradients, and on differences of differential motivation are preferable to a purely cognitive model, though they too are not quite adequate, since they do not yield a very satisfactory explanation of stereotype fixations.

The failure to formulate a satisfactory explanation of all relevant phenomena observed, not only in this experiment but generally in experiments involving the production of fixations, in terms of one of the existing "molar" behaviour theories, makes it perhaps pertinent to question the wisdom of a methodology which limits theory formation to laws about functional connections between gross environmental factors and large units of behaviour, treating organic factors in a non-commital way as intervening variables. We venture to make the purely speculative suggestion that it may be possible to explain fixations as the result of anxiety *defined in terms of the physiological anxiety syndrome*. We propose the search for an anxiety syndrome which is physiologically descriptive, instead of merely defining the concept "anxiety" as an intervening variable in a molar behaviour theory. Such an approach presupposes the collection of minute physiological data *throughout* the course of the experimental trials; e.g. change in pulse rate, breathing rhythm and other potential indices which may contribute to the anxiety syndrome, would have to be observed *inside* the experimental unit in addition to observations outside the apparatus. Observations reported particularly by Tinbergen (1951) show that complex behaviour mechanisms can often be released only by specific configurations of environmental and internal factors of which a generating relation may be a set of specific time intervals between the constituents of the configuration. Hence by establishing the magnitude of autonomic anxiety responses *before* or *after* experimental trials which involve the development of "molar" instrumental responses (e.g. in an open field test before and after a day's session), we are justified in concluding neither that what in fact is an entirely different configuration of stimuli will produce the same syndrome of responses, nor that the same syndrome would precipitate similar molar responses within quite different configurations of external and internal cue patternings. If anxiety learning is to be demonstrated, direct measurements of the anxiety syndrome *during the emission* of the instrumental responses the functional relation of which with anxiety is alleged, is desirable. Thus if increased behaviour rigidity in an insoluble problem situation were precipitated by changes of the autonomic anxiety syndrome at or near the choice point, which would revert to the previous state following an instrumental response, anxiety learning would be indicated. If it could furthermore be shown that the strength of the instrumental responses varies with intensity of the autonomic anxiety responses and with the magnitude of the difference between these responses before and after the instrumental response, anxiety learning could be expressed in quantitative laws.

The concept "frustration" could be dealt with in a similar way and redefined in terms of physiological data as a specific change resulting from prolonged exposure to anxiety. One of Maier's main contentions is that frustration-instigated behaviour is due to stress and is not motivated by drive reduction. Translated into terms of an experiment which does not exclude physiological measurements, the following sequence of events could be interpreted as confirming it: Firstly, the

typical anxiety syndrome would have to be observed after a certain period of exposure to a given stimulus situation. Secondly, after an additional period of exposure to this stimulus situation some type of determinate changes in the pattern of autonomic responses would have to occur which would (*a*) be irreversible in the sense of not disappearing after an instrumental response and (*b*) be correlated with a marked increase of rigidity of the molar response pattern. The anxiety syndrome could be interpreted as a manifestation of stress and the determinate and irreversible change in its pattern as frustration. The irreversibility of the specific change would meet Maier's criterion that frustration-instigated behaviour is not due to drive *reduction* but precipitated by the evocation of a permanent *state*.

I wish to express my gratitude to Professor R. W. Russell who supervised this research and gave constant help and encouragement. I am indebted to Professor G. C. Drew and Mr. J. W. Whitfield who have critically read the first draft of the manuscript. My thanks are due to Mr. A. R. Jonckheere for his valuable advice particularly in all matters of quantification.

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THE EFFECT ON A DIFFICULT CO-ORDINATION TASK OF THE FREQUENCY OF SIGNALS

BY

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Subjects were required to direct, by moving hand and foot controls, a fugitive spot of light into one of four positions in accordance with changes in the source of signals, and simultaneously, to extinguish two lights using a left-hand response. Two groups of subjects have been tested on this complex co-ordination task. Increasing the frequency of the signals from four successively but irregularly functioning sources was found to increase significantly the "error" scores in controlling the fugitive spot. Variations in signal frequency did not affect the other part of the task requiring left hand responses to a different set of signals. The order of a sequence of 4-signal frequencies over four trials had no effect on the efficiency with which the task was performed.

I

Introduction

Conrad (1951), employing a sensori-motor task, has investigated the effects of the frequency of signals on performance when those signals arose from a number of sources requiring simultaneous consideration on the part of the operator. Signal frequency in this sense Conrad has called *speed*, and the number of sources which give rise to those streams of signals has been called *load*. As Conrad has pointed out, speed and load qualify each other, and it is meaningless to describe a display in terms of only one of those variables. Both speed and load factors exercise a considerable effect on the efficiency with which this kind of task is performed.

In the experiments to be reported, signals occurred successively from four separate sources and therefore did not require *simultaneous* consideration, since only one source functioned at any one time. The operator was not required to *select* the source of signals demanding a response. The problem investigated, then, was the effects of variation in the frequency of successively functioning sources of signals on performance. This variable cannot justifiably be called speed in the sense that it has been used by Conrad since there exists a fundamental difference between frequency of signals from simultaneously considered signal sources, on the one hand, and successively considered sources on the other.

II

Apparatus and Method

Apparatus

The SMA-3 adapted for the measurement of the effects of frequency of signals was used throughout these experiments. An early version of this apparatus has been described by Williams (1939). Briefly, this apparatus required the control of a fugitive spot of light over a 90-second trial by means of a "joy-stick" and "rudder-bar." The speed of the fugitive spot varied during a trial and its changes of direction and speed were rapid and unpredictable. The task of the operator was to compensate for the complex input of the fugitive spot and to hold it stationary in a position indicated by a signal from one of four sources.

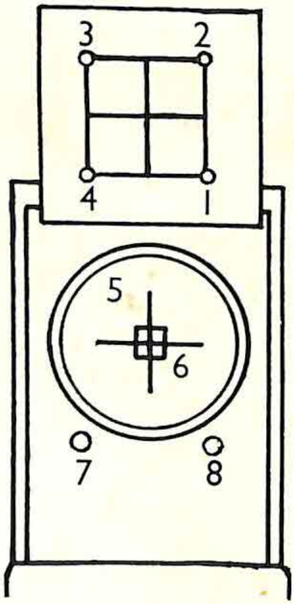
At the centre of a cathode ray tube screen, 10-inches in diameter, was marked a 2-inch square made up of four 1-inch squares and on a level with the top of this screen was a square of white card on which was marked a 6-inch square divided into four smaller squares. At the outer corner of each of these squares was a 6·5 volt lamp. In response

to a signal from one of these lamps the operator was required to adjust the moving spot on the CRT screen to the centre of the corresponding 1-inch square on the screen. For example, in response to a signal from the lamp in the upper right square the operator adjusted the fugitive spot to the centre of the upper right 1-inch square on the CRT screen, and held it there until the next signal was received. The operator's "error" score for one trial was the time for which the fugitive spot remained outside the 2-inch square. This score, then, is a measure of time rather than of the frequency, or magnitude, of a certain event. It is in this latter sense that the term error score has come to be more conventionally employed. Since it is difficult to find a more suitable and meaningful term, error score will be used to indicate time off target. The fugitive spot moved vertically over the CRT screen in response to joy-stick manipulation and horizontally in response to rudder-bar movement.

Below the screen were two lights; a red light on the right and a white light on the left. These lights flashed on seven times each in an irregular sequence during a trial and remained on, if no action was taken, for varying short periods. The operator was required to extinguish these lights using a lever situated on the left side of the cockpit. A forward movement extinguished the red light and a backward movement the white light. A metronome interrupter activated a counter while these lights remained on giving a combined score for the two lights. The responses to these two lights constituted an integral part of the total task, but for convenience this part of the task has been scored and analyzed separately.

Two scores, then, have been recorded for each 90-second trial; the time for which the fugitive spot remained outside the error-free 2-inch square, and the time taken for the left-hand responses to the two lights below the CRT screen. A schematic diagram of the display of the apparatus is shown in Figure I.

FIGURE 1



The SMA–3 adapted to measure the effect of signal frequency on performance. 1, 2, 3, 4, signal sources; 5, CRT screen; 6, 2-inch error free square composed of four 1-inch squares; 7, white light; 8, red light.

Method

The design of both experiments was in the form of a repeated Latin Square. The dependent variable was the efficiency of performance in terms of the time for which the fugitive spot was held in the 2-inch square, and the response times to the two lights below the bowl as a function of the independent variable, the rate of change of signals

from the four sources above the CRT screen. Each subject was given four trials with a different frequency of signals for each trial. The signal frequencies used were 5, 10, 15, and 20 signals per minute.

Since both the sequence and the ordinal position within the sequence of these signal frequencies could conceivably affect performance through transfer effects and anticipation, the Latin Square design was used to make the effect of these factors independent of the influence of frequency of signals. This design has made possible an analysis of variance of the two sets of scores, the performance error scores made in controlling the fugitive spot, and the combined total response times for the two lights. Throughout the analysis these two groups of scores have been treated separately.

Two experiments have been carried out. The first group of subjects were 24 University students, 12 men and 12 women, and the second group consisted of 20 R.A.F. male personnel. The second experiment was an exact repetition of the first since there was some evidence accruing from earlier work indicating that the results from the second group would not be similar to those from the first.

III

First Experiment

Procedure

Signal frequencies of 5, 10, 15, and 20 signals per minute were used. These signals emanated successively from the four sources on the panel above the cathode-ray-tube screen. The sequence of signals remained the same on every trial, but this sequence was quite irregular and reports from subjects after completing the experimental session indicated that the order was never learned and correct anticipation of the source from which a signal would arrive was not possible. Subjects were not informed of the frequency of signals before each trial. Each subject was given a preliminary practice trial during which he was required to keep the fugitive spot within the 2-inch square. The signals were not used during this trial since its function was to accustom the subject to the "feel" of the controls and the general nature of the task. Standard instructions were read out to the subject before the practice trial and questions were answered.

Results

The performance error scores made in fugitive spot control and the combined total response times to the two lights are shown in Table I. The scores in the first column of each trial are those made in controlling the fugitive spot and represent the time for which the spot remained outside the 2-inch "no error" square during one trial. For convenience these will be referred to as error scores (E). The figures in the second column of each trial are the left-hand response scores made in responding to the red and white lights during one trial (L.H.). The means and standard deviations for the four signal frequencies are given in Table II.

The data in Table I have been examined for homogeneity of variance since in replicating the same Latin Squares the pooling of the residual error sums of squares involves an assumption of homogeneity of variance. This assumption has been found tenable using M. S. Bartlett's test (Edwards, 1950).

An analysis of variance has been completed on the data in Table I and the results are summarized in Table III. The experimental design has made possible the isolation of a sum of squares attributable to the orders of presentation of signal frequencies. It is clear from the F–ratios that the order in which the signal frequencies were presented to the operator over four trials has not affected the efficiency of performance in terms of error scores or left hand response scores. Since the hypothesis of homogeneity of variance has been found acceptable a pooled error mean square has been estimated. Testing the frequency of presentation of signals against this error mean square it can be seen that signal frequency was a significant determinant of performance at better than the 1 per cent. level. This factor,

however, has had no significant effect on the left-hand response scores. The mean square for trials is significant at better than the 1 per cent. level for both groups of scores. The results of the analysis of variance demonstrate that both signal frequency and practice have a significant effect upon the proficiency of performance in fugitive spot control but the former has not influenced the left-hand response times. T-values have been computed for the differences between means for both the effects of signal frequency and practice in the case of error scores and practice for the left-hand response times. This value proved to be significant beyond the 1 per cent. level for the difference between means for 5 and 10 signals per minute and at the 5 per cent. level for all other differences.

TABLE I

Error Scores and Left-hand Response Scores for Four Signal Frequencies (Exp. 1)

Trials	1		2		3		4	
Order of presentation (Signals per minute)	E	LH	E	LH	E	LH	E	LH
5, 10, 15, 20	39	21	33	25	45	25	40	20
	38	24	32	22	48	26	46	27
	53	19	50	12	49	17	43	11
	36	36	40	31	39	31	32	21
	53	26	49	27	39	30	30	23
	31	11	30	15	25	11	26	10
20, 5, 10, 15	54	39	38	30	33	30	22	25
	72	20	60	17	58	18	61	21
	65	36	50	20	56	19	42	19
	55	33	43	28	43	22	47	27
	44	24	25	16	28	20	33	16
	44	12	25	15	17	10	36	10
15, 20, 5, 10	51	29	58	30	32	24	37	20
	53	28	45	25	50	28	40	31
	56	28	61	20	48	18	52	17
	47	32	37	28	30	32	37	17
	48	18	31	22	7	19	11	20
	64	25	52	18	47	20	62	21
10, 15, 20, 5	29	23	21	21	23	18	5	20
	46	39	34	28	34	34	25	30
	59	23	52	14	53	18	41	21
	66	24	55	20	58	13	46	18
	48	29	31	20	28	27	10	20
	45	24	25	17	45	16	15	14

A more precise impression of the effects due to the main variable can be obtained by examination of Figure 2 where mean error scores are plotted against signal frequency. Examination of this plot shows an increase in efficiency of performance as the frequency of signals demanding an adjustment of the controls is decreased. This improvement in performance is nearly linear. A linear relation between mean time off target and signal frequency is unexpected in view of previous investigations of this kind which have usually revealed a logarithmic relation. It is

reasonable to suppose, however, that since the intervals between signals were comparatively long, the logarithmic distribution has become almost linear.

Figure 2 also shows error in fugitive spot control as a function of signal frequency for the second (R.A.F.) group.

TABLE II

MEANS AND STANDARD DEVIATIONS OF ERROR AND LEFT-HAND RESPONSE SCORES FOR FIRST GROUP

Signal frequency	5		10		15		20	
(*Signals per minute*)	E	LH	E	LH	E	LH	E	LH
Mean	35·3	22·0	41·7	22·4	42·6	22·4	44·8	22·7
Standard deviation	10·6	7·3	11·9	5·53	13·1	6·5	15·8	5·4

TABLE III

ANALYSIS OF VARIANCE OF DATA CONTAINED IN TABLE II

	Error scores				*Left-hand response scores*			
Source of variation	*Sum of squares*	*d.f.*	*Mean square*	*F.*	*Sum of squares*	*d.f.*	*Mean square*	*F.*
Independent Observations—								
Order of presentation	868·30	3	289·43	0·53	61·75	3	20·58	0·14
Error	10838·05	20	541·90		2823·25	20	141·66	
Total between S's	11706·35	23			2885·00	23		
Correlated observations—								
Signal frequency ..	1277·72	3	425·90	11·19*	6·95	3	2·31	0·16
Trials	2914·22	3	971·40	25·52 *	464·04	3	154·68	11·77*
Pooled error ..	2512·31	66	38·06		868·81	66	13·15	
Total within S's ..	6704·25	72			1339·80	72		
Total for experiment	18410·60	95			4224·80	95	—	

* Significant at 1% level.

IV

SECOND EXPERIMENT

As mentioned above this experiment was similar in design and procedure to the first. The reason for carrying it out arose from previous work with this group of subjects, the results of which indicated differences between University and R.A.F. subjects on this task. Generally, as can be seen from Figure 2, the effects of the main variable, signal frequency, were essentially similar to the first group composed

of University students. The R.A.F. group composed of ground personnel did not benefit to the same extent from practice over the five trials, and the F–ratios for practice effects for both error scores and left-hand scores were not significant.

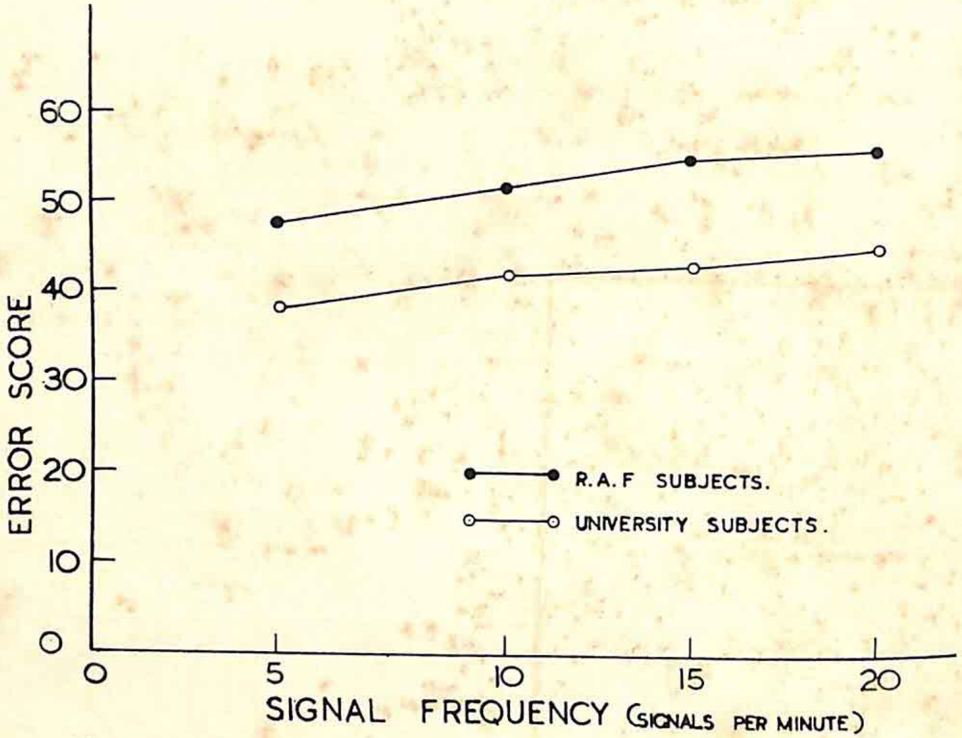


FIG. 2. Error in fugitive spot control as a function of signal frequency for two groups of subjects.

V

Discussion

The results from the two experiments indicate clearly that the frequency of signals from a visual display exercised a significant effect on the efficiency with which this tracking task was performed. In interpreting the results it is important to bear in mind that (*a*) the error score did not increase as a result of directing the fugitive spot into a 1-inch square other than that indicated by one of the signal sources, and (*b*) all subjects were unpractised on the task and only the early stages of learning have been investigated. With these points in mind it seems possible to find two reasons for the relationship between efficiency and signal frequency, although no doubt this relation is complex and more factors than the foregoing are responsible for it. First, in order to be aware constantly of the incoming signals from above the screen, and the position of the spot on the screen, the operator was required to scan the display continually. Further, he was required to attend to the lights below the screen, thus increasing the span of his scanning. With the higher signal-frequencies from above the screen he was required to take his eyes more often from the screen where the spot was rapidly moving and check the source from which signals arose. Since the changes in the course and speed of the spot were rapid and frequent the greater amount of time spent in checking the signals

at the higher frequencies probably contributed to the less efficient performance at these frequencies. At the lower frequencies the operator soon learned to anticipate the interval between changes in signal sources indicating a change in spot position, as indeed he did at the higher frequencies, and thus was able to concentrate more on controlling the spot and learning the display-control relation. Reports from subjects indicated that they became aware of the intervals to expect between changes in signal source early in the trial. A second possible explanation for the relation between frequency of signals and performance can be found in the number of decisions to action required of the operator at the four signal frequencies. During a trial the operator was required to decide how to move "joy-stick" and "rudder-bar" in order to direct the spot to the central 2-inch square as well as decide on the correct movements for the finer adjustment to one of the 1-inch squares. Moreover, he was constantly required to compensate for the erratic excursions of the spot over the screen. At the higher signal frequencies the decisions had to be made more quickly and, as a result, the subject tended to become confused and unable to cope in the time available. With frequencies of 5 and 10 signals per minute more time was available for these decisions, and a series of movements could be completed before another series was demanded by a change in the source of signal. Reports from subjects have lent support to this hypothesis and shown that the higher frequencies gave rise to confusion and indecision and inability to cope with the information from the display.

Another interesting consideration is the failure of the signals from the upper part of the display to interfere with the left-hand response times. These movements, in response to the two lights below the screen, appear to have been made independently of the signal frequencies from above the screen. This aspect of the task involved simple manual responses to the two stimuli below the bowl, and it appears that these quickly became semi-automatic and were thus little affected by changes from other parts of the display.

A final feature of interest is the effect of order of signal frequency over four trials. Subjects were not informed before each trial of the frequency of signals to expect from the four sources above the CRT screen, and it could therefore reasonably be expected that, after completing one trial with a certain frequency, anticipation or "set" could influence performance on the following trial with a different frequency of signals. The order of signal frequency has neither significantly altered performance in controlling the fugitive spot, nor in making left-hand responses to the two lower lights. It must be remembered, however, that four orders out of a possible sixteen have been considered, and it is not inconceivable that there are orders which would exercise some influence on performance.

Much advice and assistance has been given by Professor G. C. Drew throughout these experiments.

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SOME FEATURES OF THE AFTER-CONTRACTION PHENOMENON

BY

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The well-known unintentional muscular contraction following a voluntary contraction was found to be inhibited by instructions favouring relaxation, to be increased by a difficult concurrent task and to be superimposed on small voluntary contractions of the same muscles. It was not superimposed (in either sense) on antagonist contractions. An explanation is suggested, based on the interaction of sensory adaptation and some other process known to occur and apparently related to fatigue.

I

Introduction

The phenomenon of a muscular contraction appearing within a few seconds of the relaxation of the same set of muscles from a sustained voluntary contraction, and lasting up to half a minute, is well known, both as a parlour game and otherwise. Sapirstein *et al.* (1937) surveyed the literature and made a fairly comprehensive study of it. They argued that its characteristics, with respect to facilitation, reinforcement, inhibition, the strength and duration of the stimulus (the primary contraction being regarded as the stimulus), etc., resembled those of a physiological after-discharge due to a central excitatory state. Caffein and alcohol they found to increase the after-contraction, and fatigue to reduce it. These authors express their view of it as follows: "There seems no doubt that the central link in the chain of events is a persistent excitatory state in the cortical motor neurones. . . . The involuntary movement is in fact a replica of the co-ordinated movement which is necessary for its production."

The former statement means no more than that the after-contraction is caused by impulses flowing from the motor cortex (which, in any case, they do not appear to have established; there are other possible sources). The latter statement is only true in the trivial sense that the after-contraction is in the same direction as the primary and varies with its strength. Salmon (1916—quoted by Sapirstein *et al.*) thought that the phenomenon was due to a kinaesthetic image persisting in the motor cortex, somewhat in the manner of other after-images. Others have put it down to sensory adaptation, but without explaining the few seconds delay in its appearance after the cessation of the primary contraction.

Honeyman (1943—unpublished report) produced some evidence that performance in instrument flying and susceptibility to fatigue-deterioration were related to the type and degree of after-contraction given by the individual. His finding, briefly, was that both abnormally much and abnormally little after-contraction (with respect to duration or extent) were negatively correlated with good and sustained performance in certain flying skills.

II

Methods

The apparatus consisted of a handle mounted on a stiff spring, connected by a magnifying linkage to a light pointer recording on a smoked drum. The contractions employed were thus substantially isometric. The subject held the handle in his right hand and applied forces up to 8 lb. in the pushing or pulling direction as instructed. The forces used were 0 lb., $\pm 2 \cdot 5$ lb. and ± 8 lb. In instructing the subject, these were referred to respectively as zero, plus or minus one, and plus or minus two. The subjects were given some preliminary training to enable them to apply roughly the correct forces when the apparatus was screened from them; no special accuracy was demanded.

The following four different conditions were tried:—

(A) The subject was told to apply a No. 2 force—say, plus 2—and to hold it constant. After 15 seconds he was given a signal to relax it. The preliminary instruction merely warned him that he would have to hold the contraction for 15 seconds, after which he would have a short rest, and that during the rest he should keep his hand on the handle in readiness for the next part of the test. The object was to convey the impression that the primary contraction was the important thing.

(B) The instruction differed from (A) in that, instead of being told to rest after the primary contraction, the subject was specifically told to try and hold the pointer at zero—i.e. to apply zero force.

(C) The subject wrote his name backwards with his left hand while maintaining the primary contraction, and continued to do so after being told to return to zero.

(D) The subject was told "plus 2" (say); after holding this for 15 seconds, he was told "minus 1" (say); after holding this for 15 seconds, he had 15 seconds rest. Pairs of "stimuli" were given in this way until all possible combinations were exhausted.

The apparatus was screened from the subject in all the conditions. Fourteen subjects, mainly Service personnel, were tested, but time did not permit all of them to do all four tests each; they were, however, selected for the different tests at random.

III

Results

Table I gives the distribution of after-effects in the four conditions, when a primary of 8 lb. was followed by an attempt at zero force; i.e. the results for the non-zero secondaries of (D) are not included.

TABLE I

Condition	A	B	C	D
AC	11	25	24	6
Nil	30	17	1	11
AR	13	6	0	3

AR = After-relaxation or negative after-contraction.
AC = After-contraction. Nil = No clear after-effect.

Table I is significant by χ^2 at far better than the 0·0001 level (C and D being pooled to avoid small expected frequencies). Taking the conditions in pairs it is clear that this high degree of significance is chiefly due to the difference between C and the other conditions. However, A and B also differ significantly at better than the 0·01 level.

The regularity with which after-contractions were elicited in the distraction case (C) will be noted. They were sometimes as large as 20 per cent. of the primary. Occasionally the subject became aware of the after-contraction and abolished it momentarily, only to allow it to grow again.

Condition (B), in which the subject consciously tried to hold zero force, was the next most effective means of eliciting after-contractions. There were a number of responses which could not be classified either as contractions or as relaxations, and these are entered in the Table as "Nil." This means either that the pointer remained practically motionless for 15 seconds after the cessation of the primary, or that it oscillated irregularly and fairly rapidly. A slow oscillation of at least ten seconds period was interpreted according to the direction of its first peak; if it was the same direction as that of the primary, it was entered as "AC."

Condition (D) was slightly—not significantly—less effective than (B). Condition (A)—the "resting" case—was the least effective. Some of the apparent after-effects in this case may have been due to the subjects making slight body movements, which,

in view of the instructions, they would feel themselves free to do. Hence it seems fair to conclude that voluntary relaxation almost abolishes after-effects. It was thought advisable not to impress on the subjects that they should keep still, because they were to suppose that no particular interest was being taken in what they did in this "resting" period. There is reason, however, to think that this is not of fundamental importance, because trials by the experimenter and another person who knew the nature of the tests confirmed the inhibiting effect of relaxation on after-contractions; in fact, it was in this way that the effect was discovered.

Table II gives the results for condition (D), excluding the cases in which the secondary force was zero, since they have already been dealt with above. "AC" here means a movement of the pointer in the same direction as the primary, although when the secondary was of opposite sign to the primary, the movement represented in fact a relaxation of the secondary.

TABLE II

Prim.	+2			−2			+1			−1		
Sec.	+1	−1	−2	+1	−1	+2	−1	−2	+2	+1	−2	+2
AC	5	4	3	3	4	3	4	3	3	3	1	2
Nil	1	1	1	2	0	1	0	1	0	1	2	2
AR	2	3	3	4	1	2	3	3	5	5	4	4

Selecting and grouping these frequencies so as to compare large primaries followed by small secondaries of the same sign with small primaries followed by large secondaries, also of the same sign, we have Table III.

TABLE III

	$+2 \rightarrow +1$ $-2 \rightarrow -1$	$+1 \rightarrow +2$ $-1 \rightarrow -2$	
AC	9	4	$\chi^2 = 4{\cdot}1$
No AC (Nil + AR)	4	11	P $<0{\cdot}05$

Evidently after-contractions still appear, superimposed on a secondary force, provided the latter is relatively small and in the same direction as the primary. When the secondary was in the opposite direction there was no tendency for the after-effects to go predominantly one way or the other.

It was noticed that the primary forces tended to wander in a manner rather resembling the after-effects. They were therefore classified as (*a*) increasing contraction (designated "C"), (*b*) tendency to relax ("R"), and (*c*) unclassifiable ("Nil"). Table IV shows how these classes were distributed with respect to after-effects.

TABLE IV

Primary	"C"				"Nil"				"R"			
Condition	A	B	C	D	A	B	C	D	A	B	C	D
AC	5	7	2	3	1	6	4	1	3	8	18	2
Nil	6	1	0	2	5	4	0	3	14	10	1	5
AR	1	2	0	2	5	2	0	1	6	1	0	1

As before, under (D) are entered only the cases of zero secondary force. Pooling all four conditions reveals no significant relation between the form of the primary and the after-effect. But if Condition (C) is taken by itself, a relaxing primary is almost as regular a feature as the after-contraction; evidently they are both favoured by this kind of distraction. It was noticeable in all conditions, but particularly in the distraction case, that the subject tried to compensate for the tendency to relax by occasional sudden increases of force. Even omitting the distraction case, there are significantly more primaries showing a tendency to relaxation than would be expected if they were random wanderings. This, of course, is a familiar observation.

IV

Discussion

Of the suggestions that have been made as to the cause of the after-contraction phenomenon, the most popular has been that it is an example of neural after-discharge, produced, presumably, by reverberatory or delay circuits in the nervous system—in fact, that it is the same as the after-discharge found by Sherrington and others in connection with certain reflexes. But the primary contraction is not a reflex response in the ordinary sense, and the after-contraction is shown here to be strongly affected by psychological factors and by superimposed muscular activity. To identify it with the reflex after-discharge merely because it is due to a discharge which follows a previous discharge is to rely on a facile but tenuous analogy.

In particular, in the intact organism any activity which endures longer than one reaction time may be sustained by sensory feed-back (so, also, may be some that do not last so long, if they do not involve cortical analysis of stimuli). Such an activity would not be a kind of spontaneous output of the nervous system, but rather the behaviour of a cyclic system in which the environment was included.

The simplest theory that allows for sensory feed-back is that which attributes after-contractions to sensory adaptation. The subject's scale of forces, in terms of proprioceptive sensations, might be supposed to be shifted as a result of the prolonged primary contraction. Thus what would feel like zero force would be really an appreciable force in the same direction as the primary, and would be recorded as the after-contraction. But if this were the only factor, it would cause a delayed relaxation of the primary, rather than a contraction appearing perhaps several seconds after a complete relaxation.

That is to say, assuming adaptation to follow an exponential law, which is true enough for the present purpose, the after-contraction would be representable as Ke^{-at}, the origin of time being the cessation of the primary; whereas, in fact, it more resembles $K(e^{-at}-e^{-bt})$. In other words, it could be the result of two opposing processes, initially equal, and both subsiding with time. The more slowly subsiding one might be the adaptation of tension receptors. The opposing process, which must decline more rapidly, has fatigue as a conscious correlate—or, more exactly, it is reasonable to suppose that the process postulated is the recovery from that which is felt as fatigue and which causes progressive relaxation of the primary (before the signal to return to zero).

It is clear that there must be some process counteracting the adaptation of the tension receptors, because otherwise a voluntary isometric contraction intended to be of constant force would steadily increase. An increase, though far from a steady one, was found in 31 out of the 132 records (all conditions), as shown in Table IV; in fact, many of these 31 increases were too small and irregular to be attributable to adaption acting unopposed—i.e. to the maintenance of a constant impulse-frequency

from adapting tension-receptors by means of increasing force. By far the commonest pattern was a progressive relaxation punctuated by spasmodic increases of force, sometimes resulting in a cumulative over-correction.

According to the present hypothesis, the absence of consistent after-contractions superimposed on a secondary force in the opposite direction to the primary could be understood as the result of using a different set of muscles and tension receptors. Since the latter would not have to recover from adaptation, there would be no after-effect.

Likewise the virtual absence of after-contractions when the instruction is to rest—which implies relaxing as much as comfort dictates and allowing the attention to follow any other event of interest—would be explicable on the ground that, since there is no requirement to maintain any particular tension, adaptation cannot produce the wrong tension. In short, one cannot be misled by information that one ignores.

That it is principally the tension receptors, and not some other sense organs, that are responsible is only a plausible assumption as far as the present data are concerned; but they are bound to be involved somehow, and they have the necessary slow adaptation. It occurred to the writer that skin receptors might be playing some part, and he tried the effect of locally anæsthetising a forefinger and using this finger to pull the handle; the after-contractions, however, were unaffected. Matthaei (1924) obtained a similar negative result from injecting novocain into the deltoid (this was the principal muscle involved in his case, as he was using abduction of the arm from the side). No conclusions can be drawn from this, as he apparently did not test the distribution of sensory and motor blocking produced. Sufficient novocain to block all the deltoid proprioceptors would have been expected to paralyse the muscle; hence the fact that he obtained after-contractions suggests either that the muscle was not paralysed—and therefore that probably a large proportion of the proprioceptors also escaped—or that other muscles were involved. Indeed it is difficult to see how other muscles could fail to be involved.

The increase in after-contraction caused by a distracting task is presumably due to interference with the normal attempt to compensate. Sensory adaptation, like reaction time and many other distortions of information, is something we have to live with and learn to allow for as far as possible. Such compensations can be based on "dead reckoning" or on independent information (or both). In the present case, the force reaction of the handle would be transmitted to all parts of the subject's body, and therefore information from less-adapted or more quickly recovering sense organs would be available; but it would doubtless require his whole attention to make use of it. Reference has been made above to the occasional momentary abolition of the large after-contractions in this condition, which suggests that compensation, probably due to independent information rather than "dead-reckoning," is a real factor. Large after-contractions do undoubtedly reach consciousness, though moderate ones, as in Condition B, do not do so as a rule.

No attempt will be made to explain Honeyman's findings because, although it would be easy to do so either on the adaptation or on the after-discharge theory, it could not be more than speculative at present.

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TACTILE LOCALIZATION

BY

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Clinical studies suggested that errors of tactile localization made by leucotomized subjects were due to a failure of analysis of a conceptual type.

The present experiment examines the normal mechanisms of localizing stimuli to different sites on the hands by measuring the time taken to make a correct response as well as by an analysis of the errors made under conditions of stress (the reaction-time situation). It is demonstrated that healthy subjects of above-average intelligence have appreciable difficulty in determining which of their fingers has been touched. The degree of this difficulty depends on which finger has been stimulated. The results strongly support the hypothesis that normal tactile localization involves the analysis of primary sensory data in terms of a few simple parameters of orientation. It is suggested that some of the errors which occur are due to contamination between different systems of conceptual analysis.

INTRODUCTION

In a previous paper (Elithorn *et al.*, 1952), it was suggested that the distribution of errors of tactile localization made by leucotomized subjects represented a pattern of difficulty present in normal tactile localization. An analysis of the observations made on these subjects led to the conclusion that the errors which occurred were due to failures in the organization of sensory information according to certain concepts or dimensions of orientation. It was also suggested that there might be significant differences in the types of failure occurring in different subjects. The mechanism by which localizing responses to tactile stimuli are made is clearly of relevance to problems of spatial orientation. It was therefore considered worth while to investigate these aspects of tactile localization in healthy subjects.

Although tactile localization is never a perfect performance healthy subjects of average intelligence have little difficulty in referring tactile stimuli on the fingers to the correct finger and to the correct side of that finger. It is not practicable, therefore, to examine tactile localization in healthy subjects merely by analysing gross errors. However, introspection and observation of the subject's behaviour in carrying out the task suggested that stimuli at some sites were not localized as confidently or as promptly as at others. It seemed therefore that it should be possible to form an estimate of the differences in difficulty of localizing stimuli at different sites by measuring the time taken to make an appropriate localizing response, as well as by noting the errors which occurred when the subject was instructed to make a response as quickly as possible. This paper reports the results obtained with such a method.

METHODS

Experimental Technique.

Tactile localization has been studied using a method similar to that used in the previous paper and conforming to the principle of Head's modification of Henri's method. The subject was seated comfortably at a table with one hand palm downwards behind a screen. In front of him and also in the prone position was a model of a hand cut out from 18 gauge brass and electrically insulated except for the part representing the digits. In his free hand the subject held a brass stylus with an insulated handle. He was instructed to hold this stylus in contact with a cross marked at the centre point of the brass model hand,

except when actually making a response. The subject was then told that as soon as he felt a stimulus on the hand behind the screen he should contact with the stylus, as quickly as he was able, the side of the finger on the model hand corresponding to the site of stimulation on his own hand. The tactile stimulus was applied with a probe with a flat end approximately 1 mm. in diameter, so arranged that light contact of the point with the skin closed two electrical contacts. The time elapsing between this stimulus and the subject's response of touching the brass model was measured electronically. Using this apparatus records were kept of the time taken to make a localizing response, any errors which occurred and any correction subsequently volunteered by the subject.

Three healthy subjects and three psychiatric patients were the subjects for the experiment (the three psychiatric patients suffered from functional disturbances and were not considered to have organic cerebral disease). During each test session 8 different sites on each hand were stimulated. Each finger (but not the thumb) was stimulated on each side of the distal phalanx. In the case of the three healthy subjects and one patient, each of these sites was stimulated 10 times in each test session and in the case of the remaining two patients each site was stimulated 8 times. As in the previous investigation the sequence of stimuli on the fingers and also the order in which the hands were examined was balanced to obviate differential fatigue and practice effects. Each session was preceded by an unscored practice run of 16 stimuli. One of the healthy subjects (A.E.) also repeated the experiment in order to investigate the effect on a localizing response of the location of the preceding stimulus. In this investigation a sequence of 65 stimuli was so arranged that a stimulus at each site was preceded by a stimulus at each of the 8 sites including a stimulus at the site in question. No clear cut effect was demonstrated and the reaction times are not reported here. The errors made, however, have been included in the analysis of errors. Throughout the experiments reported here no indication of the accuracy of their responses was given to the subjects but learning by indirect clues cannot be excluded.

In order to exclude the possibility that the localization times obtained by the method described resulted largely from the time taken to indicate a site these "motor times" were also measured in four of our subjects. The site of each stimulus was indicated with a marker on the model hand a few seconds before each stimulus actually occurred. The subject thus knew exactly what response was required before he was stimulated. The number and sequence of stimuli was as described for the main experiment.

Statistical Methods

In demonstrating the relationships between the different patterns of times and errors we have used the correlation coefficient as the simplest measure of concordance. The methods used involve the calculation of the statistic "S" and the corresponding correlation coefficients τ and W as described by Kendall (Kendall, 1948). Except when specifically noted, a correlation derived from both hands is the mean figure derived from separate estimates for each hand. In testing the significance of the distributions of the errors at different sites we have used χ^2. For the analysis of the possible factors which might affect the direction in which errors were mislocated the ratios of errors going in each direction have been calculated and the significance of four theoretical factors tested by adopting the standard method of analysis of a factorial experiment with two treatments each acting at two levels. (Snedecor, 1946.)

Results

Reaction times.

It can be seen from Table I that in each subject the distribution of reaction times on the left hand and on the right hand are closely similar. For simplicity of presentation the reaction times at corresponding sites on the two hands have therefore been summed and the results reported in this section refer to such summed results.

The reaction times at the 8 different sites are not randomly distributed but vary with the site of stimulation and it can be seen in Table I that there is considerable similarity between the distributions of reaction times of different subjects. Thus each subject recognizes the two outside fingers much more rapidly than the other two. Further, all subjects are quickest in locating stimuli on the fifth finger and

localization is slowest on the third finger. This is the case whether the total reaction times are considered or the localization times as obtained by subtracting the mean motor time from the mean total reaction time for each subject. For while the motor time takes up a large proportion of the total reaction time at the fastest sites (digit 5) it takes up almost the same amount of time at the slowest sites (digit 3) and this is proportionately much less. In fact the mean motor time is almost the same at each site and contributes little to the overall variation between the sites. This is shown by the low value found for the coefficient of variation for the motor times compared with that for the total reaction times (Table I). It is therefore clear that the motor element in the task contributes little to the differences observed and for the purpose of discussion the total reaction times will be used.

The pattern of reaction times at the different sites shows a considerable degree of consistency with repetition of the test in the same subject and may be taken to represent a permanent pattern of difficulty. The close resemblances mentioned above between the patterns provided by the reaction times of different subjects is revealed by the rank correlations (w) between different subjects on each run. The concordances between subjects on each run give a mean value of $w = 0{\cdot}749$ ($p < 0{\cdot}001$). On the basis of the contingency of reaction time on the site stimulated, the similarity of the reaction time distribution with repetition in the same subject and the similarity of reaction time distribution in different subjects it is fair to conclude that uncertainty in making a localizing response on the fingers is primarily a function of the site of stimulation.

Incidence of errors.

Incidence of errors made by our six subjects is shown in Table I and in Figure 1. Of these 538 errors, 236 or 43 per cent. were spontaneously corrected by the subjects immediately the wrong response had been made and this suggests that these errors were largely the product of the stress of the reaction time situation. However the

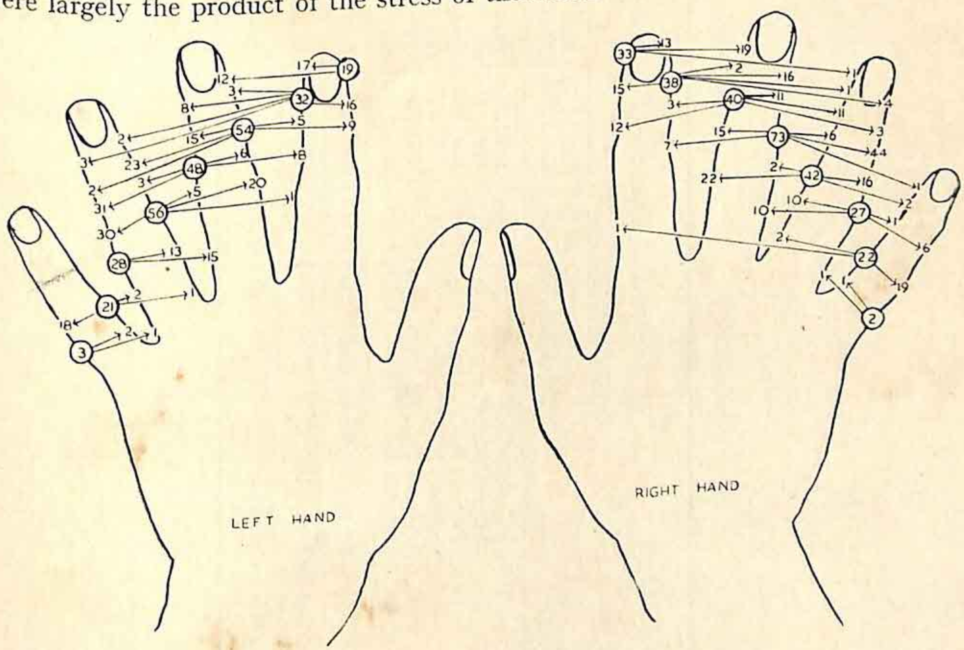

FIG. 1. Incidence of errors and their sites of referral for all six subjects.

TABLE I

MEAN REACTION TIMES AND TOTAL ERRORS

	Left Hand										Right Hand							
	Finger 5		Finger 4		Finger 3		Finger 2		Co-efficient of Variation	Co-efficient of Variation	Finger 2		Finger 3		Finger 4		Finger 5	
	Uln. Side	Rad. Side	Uln. Side	Rad. Side	Uln. Side	Rad. Side	Uln. Side	Rad. Side			Rad. Side	Uln. Side	Rad. Side	Uln. Side	Rad. Side	Uln. Side	Rad. Side	Uln. Side
Mean Reaction Time Subject M.A.C.	528	536	875	796	840	740	582	594	20·70	24·02	646	593	1062	882	804	790	570	565
" " " Subject M.F.P.	462	454	874	811	1180	1024	695	563	34·70	19·95	577	696	873	779	737	615	501	466
" " " Subject A.E.	446	553	1349	1210	1885	1964	896	1446	46·12	43·30	1416	818	1732	1724	1007	1162	562	497
" " " Subject M.S.	1127	1330	1213	1708	1962	1737	1558	1194	20·71	13·22	1477	1509	1509	1782	1547	1260	1394	1147
" " " Subject B.G.	968	1212	1547	1782	1697	1728	1303	1381	19·71	9·46	1330	1489	1680	1569	1515	1451	1346	1263
" " " Subject E.S.	1191	1498	1876	2021	2349	2573	1850	1716	23·47	27·89	1656	2165	2320	2993	1948	1816	1374	1344
Mean Reaction Time for all subjects	787	931	1289	1388	1653	1628	1147	1149	24·68	20·50	1184	1212	1529	1622	1260	1183	958	880
Mean Motor Time for four subjects	472	476	482	513	498	503	508	483	3·15	3·97	507	493	515	523	545	545	510	492
Total Errors made by all subjects	3	21	28	56	48	54	32	19	56·97	57·71	33	38	40	73	42	27	22	2

incidence of these errors may be taken to reflect the comparative difficulty at different stimulus sites of one aspect of the task. It can be seen (Table I) that although there is a good measure of agreement between the pattern of difficulty suggested by the incidence of errors and that suggested by the reaction time results, nevertheless concordance is by no means complete ($\tau = 0{\cdot}75$). This would support the hypothesis that subjective uncertainty is not directly dependent on the probability of making an error.

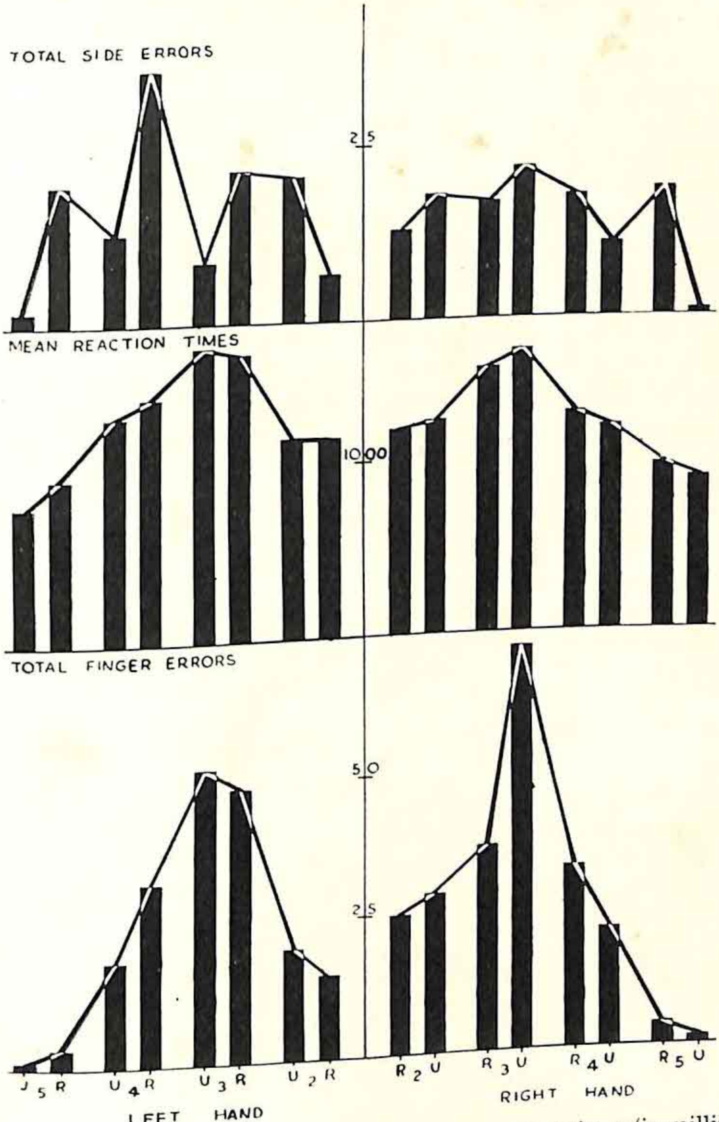

FIG. 2. Histograms to show relationships between reaction times (in milliseconds), incidence of finger errors, incidence of side errors and the site of stimulation.

In a previous paper we reported that errors of finger localization were of two main types which were largely independent. These two types of error were mislocalization of the finger stimulated and mislocalization of the side of the finger stimulated. It was suggested that these two concepts, or dimensions of orientation, might play a part

in normal tactile localization. The results of the present experiment support this. In Figure 2 the distributions of two types of error are shown separately. These errors are respectively:—(1) All errors involving mislocalization of the finger stimulated, including those involving both mislocalization of the finger and mislocalization of the side of the finger and (2) All errors involving mislocalization of the side of the finger stimulated including those involving both mislocalization of the side of the finger and mislocalization of the finger. As in the previous investigation there are marked differences between these two distributions. The difference between the distributions of these two types of errors in the case of the total errors made by our six subjects gives a χ_2 of 62·30 which with 7 degrees of freedom gives a chance probability of less than 0·001. Finger errors occur much more frequently on the central two fingers (76 per cent.) than on the two outside fingers and most frequently of all on digit 3 which, if the thumb is included, may be regarded as the centre of the hand. Side errors however occur with almost equal frequency on each finger but with different frequencies on each side of a particular finger. Thus side errors are more frequent (65 per cent.) on the insides of the fingers (ulnar sides of digits 2 and 3, radial sides of digits 4 and 5). These differences are illustrated diagramatically in Figure 2.

Further evidence supporting the suggestion that finger localization and side of finger localization occur independently is provided by the incidence of errors involving mislocalization of both finger and side of finger. If errors were referred to the alternative sites simply on the basis of the distance of these sites from the site stimulated or on a purely random basis, then the frequency of the compound error described should be approximately the same as that of errors involving mislocalization of the finger alone. This is far from being the case; of the 538 errors made by our subjects, 53·7 per cent. are simple finger errors and 38·5 per cent. are simple side errors; only 7·8 per cent. are compound errors. Thus it may be said that the incidence of combined errors is very much less than would be expected if "finger" localization and "side" localization were skills which were not performed with some measure of independence.

Relationship Between Incidence of Errors and Reaction Times

On the basis of our conclusion that finger localization and side of finger localization are skills which are performed separately we may now enquire what relationship obtains between the pattern provided by the reaction times at the different sites and the patterns provided by the finger errors and side errors respectively at the different sites. These patterns are shown graphically for the group of six subjects in Figure 2. As has already been pointed out congruity between distribution of reaction times and distribution of all errors is not complete. However, when the reaction time pattern is compared with the pattern of finger errors and with the pattern of side errors separately the relationship becomes clearer. It can be seen in Figure 2 that the pattern of reaction times resembles the distribution of finger errors more closely than it resembles the distribution of side errors. Both the reaction times and the frequency of finger errors show considerable variation from one finger to another, but comparatively little difference between the two sides of each finger whereas in the case of side errors there is greater difference between the two sides of individual fingers than exists between different fingers. Hence the concordance between the reaction time pattern and the finger error pattern ($\tau = 0{\cdot}780$) is higher than that between the reaction time pattern and the side error pattern ($\tau = 0{\cdot}500$). In other words, the time taken to make a localizing response gives a far better indication of the probability that a finger error will occur than that a side error will occur. This

would seem to suggest that subjects take longer to decide which finger was stimulated than which side of the finger was stimulated. Further, five out of six of our subjects made more finger errors than side errors. In all these five subjects the reaction time pattern resembled the finger error pattern more than it resembled the side error pattern. In the one subject who made more side errors than finger errors the reverse is the case. Thus both the reaction time method and the frequency of errors suggest that most subjects find it more difficult to decide which finger has been stimulated than which side of the finger has been stimulated.

Direction in which errors are referred.

The total incidence of errors and the sites to which the stimuli are referred is shown in Figure 1. If it is accepted that the two types of error occur to a large extent independently then the factors which govern the direction in which displacements occur should be considered separately for each of the two types of error. In the case of side errors the direction of referral is fully determined by the site of incidence. Thus a simple side error occurring on stimulation of the ulnar side of a finger is necessarily a misplacement in a radial direction. Similarly a side error following radial stimulation of a finger must involve a displacement in an ulnar direction. With compound errors the displacement resulting from the side component will be similarly determined, although the direction of overall displacement will depend on the direction in which the finger is displaced.

The direction of displacement of a finger error following stimulation of digits 2 and 5 is also fully determined. If a finger error occurs on digit 2 the displacement will be ulnarly. (Theoretically errors on digit 2 could be referred to the thumb. Previous studies with leucotomized subjects have shown that stimuli were only referred erroneously to the thumb when the subject was grossly confused and such errors never occurred in the present series). Similarly, finger errors on digit 5 will be displaced radially. Errors on digits 3 and 4 can, however, be displaced in either direction. Inspection of Table 2 or Figure 1 shows a tendency for finger errors to be referred away from the nearest border of the hand, i.e. for errors on digit 3 to be referred ulnarly and those on digit 4 to be referred radially. On the left hand 82·0 per cent. (100) of errors are referred centrally and on the right hand 76·2 per cent. (99).

The fact that an error is referred centrally rather than towards the borders of the hands is unfortunately only one of several possible ways of verbalizing the same observation. Inspection of the task suggests that there may in fact be several factors influencing the direction of error referral. In fact the possible finger errors at these stimulus sites could mathematically be accounted for in terms of four factors. Of these four factors three could be regarded as having physiological significance. Each of these three factors, which are discussed below, might be interpreted physiologically in more than one way. The relative importance of the theoretical factors is open to statistical examination, although it is impossible thus to distinguish between the possible physiological interpretation of each factor.

Factor F. (Freedom Factor).

(*a*) Errors are more likely to be referred by chance in the direction in which there are a greater number of possible sites available.

(*b*) Errors are likely to be referred either away from or towards the borders of the hand.

(*c*) Errors might tend to be referred to the sites at which most errors occur.

While these tendencies may be of opposite sign and therefore oppose one another they would not have distinguishable effects at the four possible sites. As a group

TABLE II

DIRECTION OF DISPLACEMENT OF FINGER ERRORS FOR FINGERS 3 AND 4

Left Hand		*Finger 4*		*Finger 3*		*Estimated strength of factor for both hands*	*Finger 3*		*Finger 4*			*Right Hand*
		Ulnar Side	*Radial Side*	*Ulnar Side*	*Radial Side*		*Radial Side*	*Ulnar Side*	*Radial Side*	*Ulnar Side*		
Finger errors displaced ulnarly		0	0	34	25		14	51	2	7		Finger errors displaced ulnarly
Finger errors displaced radially		15	26	8	14		15	7	24	10		Finger errors displaced radially
Proportion of errors going ulnarly		0	0	0·81	0·64		0·48	0·88	0·08	0·41		Proportion of errors going ulnarly
Suggested action of factors	F	→	→	←	←	79·0*	→	→	←	←	F	Suggested action of factors
	S	→	→	→	→	58·7*	←	←	←	←	S	
	C	←	→	←	→	61·3*	←	→	←	→	C	
	X	→	←	←	→	52·9	←	→	→	←	X	

* Significant effect.

they have been provisionally labelled "freedom factor" in accordance with the first interpretation.

Factor S. (Spatial Factor.)

(*a*) Errors might tend to be displaced in a constant direction in relation to the hand, i.e. ulnarly or radially.

(*b*) In relation to the body as a whole, i.e. to the left or to the right.

While these two spatial tendencies are confounded on the individual hand they might be distinguished by differing effects on the left and right hands, i.e. an overall tendency to displace stimuli to the right would on the right hand reinforce a tendency to displace stimuli ulnarly whilst on the left hand the two tendencies would be in conflict. This factor is provisionally labelled "spatial factor."

Factor C. (Contamination Factor.)

(*a*) A tendency to displace finger errors in the direction corresponding to the side of the finger stimulated. This could result from contamination of the orientation of the stimulus with respect to the fingers on the hands by the orientation of the stimulus with respect to the side of the fingers.

(*b*) A tendency to displace the finger error to the finger next nearest to the site of stimulation.

This factor has been provisionally labelled "contamination factor" in accordance with alternative (*a*). As previously mentioned the direction in which finger errors are referred is uniquely determined on digits 2 and 5 and it is only therefore on digits 3 and 4 that the effects of the above factors can be observed. On these centre two digits there are 4 sites of stimulation and from each of these sites it is possible for errors to be referred in either of two directions, giving in all eight possible types of observation. If it is assumed that each of the above three factors "F," "S" and "C" acts at these 4 sites and that at each site each factor exerts its influence at two "levels" favouring the reference of errors in one direction and tending to inhibit them in the other then at each site the three factors will produce a different combination of effects.

Factor X.

The above three factors do not exhaust the simplest set of factors which might determine the direction of errors. There is a fourth factor which could conceivably contribute to the observed results but which does not correspond to any obvious function. It can be regarded as residual or error factor, and has been called Factor "X."

These four factors will tend to increase or decrease the proportion of errors going in the ulnar direction, according to the scheme of arrows in Table 2. The observed proportions of ulnarly directed errors occurring at the four stimulus sites have been analysed to provide estimates of the strengths and directions of the four factors. The analysis is analagous to that of a factorial experiment with two "treatments" each at two levels (Snedecor, 1946). One "treatment" would correspond to the contrast between fingers 3 and 4, and the other "treatment" to the contrast between the ulnar and radial sides of a finger. The factors F, C and X correspond respectively to the main effects due to fingers, that due to sides, and the interaction between fingers and sides. The factor S corresponds to the difference between the mean observed proportion and the mean expected proportion (0·5). The strength of each factor has been expressed by estimating the percentage of errors which, at each site, would be referred in the direction in which the factor exerts its effect, if that factor

were acting in isolation. The significance of the departure of this percentage from the "null" value of 50 per cent. is tested by applying standard formulae for the standard errors of the observed proportions. For example, at sites 3R and 4R factor C will tend to decrease the proportion of errors going ulnarly while at 3U and 4U the factor will increase the proportion of errors going ulnarly.

Because of the small number of observations the two hands have been considered together. Factors F and C and S are found to be significant at the 0·05 level of confidence whilst factor X is insignificant. Thus the observed results are consistent with the suggestion that the three factors for which a functional basis can be suggested exert significant and independent effects and that the remaining possible sources of variation namely experimental error or interactions between the other factors are insignificant.

Of the three significant factors, F is the strongest and would, if it acted alone, cause 79 per cent. of errors to be referred in the direction of its effect. Similarly, Factor C acting alone would produce a proportion of 61·3 per cent. and Factor S 58·7 per cent. The error factor, however, acting alone, would cause only 52·9 per cent. of errors to be referred in the direction of its effect. The direction of these effects is shown by the arrows in Table II.

Discussion

It is clear that subjects of normal intelligence free from known physical or psychiatric disease have appreciable difficulty in locating tactile stimuli on the fingers. The types of error which have been observed suggest that much of the difficulty results from faulty conceptual analysis of the information provided by stimulation. The observation that finger errors and side errors occur to a large extent independently suggests that under the condition of this experiment not less than two separate frames of reference are utilized by a subject in orientating a stimulus on the fingers. Further support for this is provided by the occurrence of errors which are consistent with the hypothesis that confusion may occur between these two frames of reference [Factor C].

Hick (1952) has recently shown how in a multiple-choice situation the reaction time increases with the number of choices presented. If our suggestion is accepted that finger localization and side localization are separate intellectual performances then the present task may fairly be regarded as a composite of at least two multiple-choice situations. Finger localization will be a four (or five if the thumb is included) choice situation and side localization a two-choice situation. Thus Hick's hypothesis would demand that finger reaction times should be longer than side reaction times. Our observations are consistent with this in that the reaction times reflect the difficulty of deciding which finger is stimulated better than they do the difficulty of deciding which side is stimulated. However, one of our findings to which Hick's theory cannot be applied is the observation that more errors occur on some fingers than on others and that more errors occur on one side of fingers than on the other. In the classical choice reaction time experiment the choices are presumed to be of equal difficulty but this is clearly not the case with tactile localization. We suggest that the unequal distribution of errors and the variation in reaction time at the different sites results, not from primary sensory differences but from the nature and complexity of the perceptual analysis called for by stimuli at different sites. This would be consistent with our suggestion that perceptual local sign involves reference to a number of parameters. Thus in the case of finger errors we have found that error frequency as well as the reaction time is related to the distance of the stimulus site from the borders of the hand. This observation encourages the hypothesis that

the pre- and post-axial borders of the hand are actually used as reference points in localizing the finger stimulated. It should be mentioned that the borders of the hand could be used as reference points in two ways. Either there might be an ulnar-radial dimension with pre- and post-axial borders as the two poles of the dimension or there might be an inside-outside factor with the two borders together constituting one pole and the centre of the hand the other pole. If the thumb is excluded (and it will be remembered that in healthy people stimuli on the thumb are never referred to another digit) these two dichotomous dimensions could together define the finger stimulated.

These reference points may also be used in deciding the side stimulated, and the greater frequency of errors on the insides of the fingers (ulnar sides of digits 2 and 3 radial sides of digits 4 and 5) supports this. It seems likely that the poles of each dimension used are not equipotent. Examples have already been cited in the case of side errors, and in the case of finger errors more errors occur on the inside fingers of the hand than on the outside ones and more occur on the ulnar fingers than on the radial ones.

Finally comment should be made on the direction of reference of errors. Of the three factors which have been studied in the case of the finger errors, two call for little comment. The tendency for stimuli to be referred in the direction in which most alternative sites are available may be taken to represent the random component in determining direction of errors. The tendency for stimuli to be referred in the direction corresponding to the side of the finger stimulated we have suggested to be a contamination between two frames of reference, namely the finger stimulated and the side of the finger stimulated. This we believe to be an effect of some importance in spatial orientation but we have acknowledged that this finding could be interpreted in another way. However, in support of our suggestion is the fact, pointed out in an earlier paper, and again recorded here, that the direction of reference of side errors is also consistent with such a "conceptual contamination." Side errors referred ulnarly are more common on the ulnar fingers of the hand and side errors referred radially are more common on the radial fingers of the hand. This interpretation involves the assumption that the determination of the side stimulated and of the finger stimulated involve the use of at least one dimension in common.

The third tendency in the case of finger errors is the constant spatial error; the tendency for the finger to be mislocated in an ulnar direction. Such a constant spatial error has already been demonstrated by other workers but their findings have provoked little comment. It has been demonstrated (Hulin, 1935) that the direction and extent of a spatial error bears a relationship not only to the position in space of the part stimulated, but also to the position of the model on which localization occurs and to the direction of movement of the part stimulated if this part is stimulated when in motion. To the present authors this again suggests "contamination" between two systems of orientation. It seems very likely that spatial errors which vary with the position of the model relative to the site stimulated and with the direction of movement of the stimulated part result from confusion or competition between information concerning orientation in external space and information concerning orientation on the skin surface. It should be pointed out that even localization direct to the site stimulated (provided knowledge of results is controlled) constitutes localization to a model which has the same position in space but not in time as the site stimulated. Even position in space is not constant if the part stimulated is moved before localization occurs. It is reasonable to expect both tactile and proprioceptive components in the kind of orientation known as local sign, and when these components provide information which is not mutually equivalent

(i.e. when movement of the part stimulated takes place between the time of stimulation and the time of localization, or when localization takes place to a model which does not occupy the same position in space as the part stimulated), then the contaminatory effects commonly observed in the sensory rivalry situation (Asch and Witkin, 1948) are to be expected.

We would like to express our gratitude to the three patients who assisted us in the reaction time studies, and to thank Dr. Eliot Slater for his permission to obtain their help. To Dr. E. A. Carmichael and Professor O. L. Zangwill we are indebted for helpful criticism and advice. Dr. Armitage of the Medical Research Council's Statistical Research Unit showed us how best to analyse the factors concerned in error referral and kindly undertook the necessary calculations. In addition we owe him our thanks for considerable help with the other statistical methods used.

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THE AFTER EFFECT OF SEEN MOVEMENT ON A PLAIN FIELD

BY

G. C. GRINDLEY AND R. T. WILKINSON

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Twenty laboratory subjects were asked to fixate the centre of a rotating spiral disc, under conditions where all of them, on stopping the disc, obtained the well known illusion of apparent expansion outwards. In this experiment, instead of stopping the disc, a completely uniform white surface was substituted and it was found that though the borders of the surface appeared to remain still, the surface itself showed apparent expansion in nearly all cases.

Introduction

The after-effect of seen movement is a phenomenon which has long been known, but of which the explanation remains obscure. Probably the best general account is still Wohlgemuth's monograph (Wohlgemuth, 1911), though other writers (e.g. Granit, 1928) have since examined in more detail the influence of some of the relevant factors. Those features of the effect which deserve mention here are (*a*) its "ghostly" character, in that objects appear to be moving yet not changing their position, (*b*) its localization to the area of the field of view stimulated and (*c*) the transference of the effect to quite other objects if they are substituted in the same part of the field.

The problem discussed here is "What will happen if, after viewing a moving pattern, the eye is presented with a *completely* uniform white field in the relevant area?" Before doing the experiment several of the subjects suggested what *ought* to happen, e.g. there was the rather unlikely suggestion that the stimulus pattern should be seen (as a negative after-image) but with a reversal of its direction of movement.

Apparatus and Procedure

The apparatus was simple. An ivorine disc 8 in. in diameter was mounted on the shaft of a gramophone motor rotating at about 78 r.p.m. On the disc was painted a black spiral of four turns; both the black lines and the intervening white space being about ½ in. wide. This spiral was viewed through a low-powered telescope, arranged so that the disc completely filled the field of view. With the particular telescope used, the field of view at the subject's eye was 60° visual angle in diameter.

In order to obtain a uniform physical stimulus to the whole area, a white shutter just in front of the object glass of the telescope was closed after the end of the exposure. This is the principle of Maxwellian view as used in a tachistoscope (*cf.* Grindley, 1933). All the subjects agreed that in viewing this shutter before any exposure of the rotating spiral they saw a completely uniform white field. The shutter was opened and the subjects then saw the rotating spiral for 15 seconds, after which the shutter was closed. (In preliminary trials with each subject the spiral had merely been stopped, but remained in view.)

Results

Of the twenty subjects used, all of whom saw the after effect after the spiral *stopped*, nineteen reported that when, instead, *the shutter was closed*, they saw the white surface expanding or coming nearer to them. An interesting feature of the reports in nearly every case was that what had previously been merely a plain white surface now appeared to have some kind of structure in it. It was never reported as a spiral, and typical comments were that it looked like a rice pudding getting

nearer to you during a fog, or like some kind of linen sheet expanding. This kind of vague structure imposed by the subjects on a uniform field vanished in all cases when the after-effect ceased. One is almost tempted to call it an example of "rationalization," i.e. that the subjects had a feeling of expansion in the visual field and, if there were expansion, there would have to be *something* to expand. It should be noted that this effect was a surprise to most of the subjects, but that there was only one out of twenty who did not spontaneously report it.

Conclusion

The experiment described here seems merely an addition to the phenomenology of the after-effect of movement. The explanation is as obscure as ever.

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BOOK REVIEW

Brett's History of Psychology. R. S. Peters (Editor). Abridged edition in one volume. London, Allen & Unwin, and New York, Macmillan. 1953. Pp. 742. 42s. net.

Brett's *History of Psychology* is, as we may agree with his present editor, a "classic," and like other classics it is probably less often read than it deserves. For this its inaccessibility is partly responsible, and on this score alone psychologists and others besides will be grateful to Messrs. Allen & Unwin for making it easily available at a figure which recent book prices suggest is not unreasonable. But now another mark of the "classic" has been impressed upon it. It has been edited.

Brett is in many ways an unusual book. Not only does it cover the history of psychology along lines not attempted by other works, but for all its length it is easy and enjoyable reading, and gives the pleasing and altogether too rare impression that the author enjoyed writing it. Its erudition, unobtrusively and tastefully deployed, is a means and not an end in itself. Nor does Brett try to select and interpret the events of the past merely from the standpoint of their outcome in present pre-occupations. Rather he is engaged in a leisurely exploration of highways and byways for their own sake. This is by no means to say that his work lacks aim and structure. Uncramped by over-explicit or categorical analysis, Brett persuades the material of each period to reveal the moulds in which it is cast. The reader feels free to take what is expounded as he wishes. No historical or philosophical doctrine as to its significance is being thrust upon him.

Another feature of Brett's *History* with some bearing upon its re-issue in a form intended for the ordinary student's use is its rather curious pyramidal shape. The fulness of content and treatment tapers off as we approach the present day. This doubtless in part reflects the emphasis of Brett's own interests: in part it is due, as he points out, to the fact that "its problems cease to be capable of purely historical treatment as one approaches the most recent period." It is a tribute to his skill that the result leaves no sense of ungainliness. But it will be clear that the decision to re-publish the book in an abridged form must make very severe demands upon the wisdom and skill of the editor, if the rather unusual and attractive flavour of the original is to be preserved.

Of various courses open to him, Mr. Peters has chosen to omit some sections, to write in abbreviated linking passages, and to add a final chapter indicating twentieth century trends. (The first edition was completed in 1921). He has been scrupulous in distinguishing typographically between Brett's own words and those of himself and his collaborators. At the end of the volume a table headed "List of major omitted sections" is provided. This is less revealing, and less scrupulous, than it appears. Comparison with the original edition seems to show that omissions have been entered in this table only when they involve a whole section as numbered by Brett. Some such sections are short and unimportant. But other parts have disappeared which, since they do not coincide with one of Brett's own sections, are unaccounted for in the table. Some of these are of major importance and considerable length. This fact is given inadequate, in fact negligible, prominence by the editor.

It would be strange if every reader agreed with any editor's choice for deletion—a choice which doubtless had to be made within rigid quantitative constraints. But there is more than a suggestion here that the demands of contemporary fashion have sometimes prevailed over historical sense, especially in the abridgement and re-arrangement of Brett's third volume. Wundt, G. E. Müller and Stumpf, for instance, are now deprived of systematic treatment. Yet each of these has some place in history—if only as a measure of the nature of their successors' revolt. As Brett himself remarks *à propos*, "Progress is a kind of critic; but it does not despise the things it must discard." To which it might be added that Progress would be very unwise if it did. The omission of the section on Lotze's *Medicinische Psychologie*, again, seems to betray some lack of appreciation of nineteenth-century continental tradition—a tradition within which Freud was reared and from which derive some features in his theories often puzzling to English readers. Throughout the third volume the editor has quite extensively re-ordered the material. The result he has endeavoured to cement with fresh chapter headings and interposed passages whose didactic tone is not always at ease with the spirit of the original.

It is clear that, however extensive a re-arrangement is carried out, there could be no question of "completing" Brett's work so as to make it reach to the present day. The editor has wisely omitted the whole of the author's final chapter, and has sought to round

off the edition with a new one in which some of the main themes of the twentieth century are delineated by himself and two collaborators. It can well be imagined that this was a difficult task, deserving more time and patience than the writers seem to have had at their disposal. Though the new chapter comprises some thirty thousand words, they are not economically employed. Much space is devoted to the work of prominent individuals such as Hull, Freud, McDougall, and the Gestalt Psychologists. Most of this is unexceptionable if elementary; it could reasonably be expected of Brett's readers that they will be well-acquainted with the material at this level; or, if not, would look for it in one of the many similar accounts, such as Woodworth's *Contemporary Schools*. Little attempt has been made to look behind psychology's contemporary *persona*—to detect connections of ideas and trends which, though at present little in active evidence, are implicit and probably significant for the future. Bergson, for instance, introduced themes and problems into psychological discussion whose very unpopularity today suggests that they have been repressed rather than surmounted, and may crop up again in other and more fruitful forms. Bergson had a place in Brett's last chapter, but finds none in this edition. The influence of W. H. R. Rivers and J. T. MacCurdy, though perhaps largely personal, was such that without some knowledge of them it would be difficult to give a reasonable account of a not unimportant part of recent psychology in this country. Their names do not appear, a distinction which they share with Kraepelin, Janet, Rorschach, Michotte, Piéron and Goldstein. All of these, in their different ways, have contributed significant elements to that diversity in the psychological scene which Brett would surely have been quick to seize as a condition of future progress.

To assess the impact of physiological and biological work upon contemporary psychology is perhaps the most difficult task of the final chapter in this edition. It is a pity that more space was not devoted to it, and that the section is marred by an ill-informed background, some downright mis-statements of fact, and a number of obscurities of expression. When we read, for instance, that the "histologist" Granit "has shown that nerve fibres from the retina fall into three classes, all behaving very differently," there springs to mind the probability of some inadvertent, if inexcusable, confusion between Granit's analysis of the ERG into three components, and Hartline's distinction between "on," "off," and "on-off" elements—with the likelihood that the latter are the entities intended. But what springs to mind when we read that "terminating behaviour appears invariably to be an act not always obviously connected with the initiating state" defies analysis, unless indeed, the consummation referred to is the peace that passeth all understanding.

Altogether, it can scarcely be said that the editorial work on this re-issue of a solid, modest, likeable book is worthy of the original version. There is a certain pretentiousness about the editor's "methodological framework" which ill accords with unnecessary carelessness in small matters. (A casual glance at the index of proper names, for instance, reveals a half-dozen and more misprints.) It was a good thought to re-issue *Brett*, but it may be that in twenty years time copies of this version will languish on the second-hand shelves, unregarded by seekers of the original three volumes.

R. C. O.

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